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SYMPOSIUM

INTRODUCTION*

CARLYLE F. JACOBSEN

State University of Iowa

The members of this panel have addressed themselves to various aspects of and problems relating to community mental health services. Under the provisions of the National Mental Health Act passed by the Congress in 1946, a major fraction of the funds appropriated are distributed among the states for use at the state level in establishing and encouraging community mental health activities. This is a bold undertaking and one that is rich with opportunities for psychology and the mental health team, as well as one that presents many unresolved problems.

Our traditional approach to the task of improving mental health in our communities has focused on the individual, his problems and perplexities. Treatment has necessarily and wisely had its primary orientation toward helping the individual to meet his problems more adequately and to live more comfortably in his relations to his family, the school, his church, his job and the community. Since the passage of the Mental Health Act, many communities throughout the country have established new clinics or strengthened existing facilities to make this type of treatment more readily available to its citizens. With these undertakings have arisen such questions as: What is the most effective way of helping a community to appreciate the need for better mental health services? How can such desire be translated into action establishing adequate mental health services, and finally, what steps can be taken to assure their continuing growth and success in serving the people?

Our thinking has traditionally been centered about the individual, and treatment has been so oriented, but what of a positive, preventive program in the mental health field? From time to time mental health workers in the schools, community clinics, community social service agencies and the courts have been concerned with a broader approach, looking toward the prevention of mental ill-health as well as its treatment. With proper regard for such pioneer efforts, we must still recognize that we do not have an effective preventive program in the field of mental health. It is an opportunity for research, an opportunity which, if accepted and acted upon, can yield enormous dividends to our country. A positive mental health program offers our greatest opportunity, and confronts us with the most difficult task in the entire mental health field. Certainly the psychologist as a member of the mental health team, must be prepared to accept his full responsibility for research and development of new techniques and new concepts.

It is to selected facets of these opportunities and tasks that our speakers this morning have directed their attention.

* Chairman's Introduction to a Symposium on Psychology in the Field of Community Services, American Psychological Association, Division of Clinical and Abnormal Psychology, Denver, Colorado, September 6, 1949.

THE COMMUNITY SERVICES PROGRAM OF THE NATIONAL INSTITUTE OF MENTAL HEALTH, U. S. PUBLIC HEALTH SERVICE*

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Following World War II, congressional hearings revealed that various mental and emotional disorders and inadequacies accounted for the rejection or discharge of 2,000,000 men by the armed services. Moreover, despite the large number screened out before induction, mental disorders among veterans accounted for as much as a third of all pensionable disabilities and over one half of hospitalizations in the Veterans Administration. Concern over this cost to the nation in manpower losses and veteran care led Congress to study the mental health problems and needs of the country.

It was found that more than half of the patients hospitalized in the United States on any given day—some 600,000—were mental patients, and that about 8,000,000 individuals in the country were suffering from some mental disorder. No adequate programs for the prevention and early treatment of mental illness existed anywhere in the country. There were thought to be about 600 out-patient mental health clinics in the country against a minimum need for 1,400. Five states were without any public or private mental health services outside of state hospital care. The foremost authorities in the country testified that the total number of all the psychiatrists, clinical psychologists, psychiatric social workers, and psychiatric nurses in the country amounted to only about 12,000 individuals, or considerably less than the minimum number needed in any one of the four disciplines; that training facilities were adequate to train only a small fraction of the needed personnel; and that the skills and scientific knowledge required to attack seriously the mental health problem on a nationwide basis were wholly inadequate.

To help remedy this situation, Congress passed the National Mental Health Act in July 1946. The legislation gave the Surgeon General of the Public Health Service responsibility for developing a broad program to improve the mental health of the nation through: (1) training mental health personnel; (2) research in the problems of mental illness; and (3) development by the States of preventive mental health programs in their communities.

This program is administered by the National Institute of Mental Health, one of the National Institutes of Health of the Public Health Service. The major objectives of the program are: (1) to develop economical methods of bringing mental health care to persons in need of such assistance; (2) to develop preventive and control methods in the mental health field; and (3) to promote positive mental health.

A National Advisory Mental Health Council consisting of leading scientific and medical authorities is provided for in the National Mental Health Act. This body advises the Surgeon General on such matters as policy, the distribution of grant funds, personnel standards, and other problems relating to the program. The present members of the Council are: Dr. Franz Alexander, Dr. Leo H. Bartemeier, Dr. Karl M. Bowman, Dr. Alan Gregg, Dr. S. Bernard Wortis, and our symposium chairman, Dr. Carlyle Jacobsen. Assisting the Council are committees on community services, research, and training. The Training Committee has four subcommittees in each of the following fields: clinical psychology, psychiatry, psychiatric social work, and psychiatric nursing. The research committee is likewise multidisciplinary including repre-

* Presented at the meetings of the American Psychological Association, Denver, Colorado, September 6, 1949, in the Division of Clinical and Abnormal Psychology's Symposium on Psychology in the Field of Community Services.

sentatives from the various medical, biological and social sciences. On the Community Services Committee are representatives of the general public as well as the mental health professions. In addition, the Institute has several panels of consultants which advise on scientific and professional matters, program planning, and special problems. In brief, every effort is made to use the best advisory and consultant resources available.

The National Institute of Mental Health is directed by Dr. Robert H. Felix and consists of the following branches: the Training and Standards Branch, the Research Projects Branch, the Biometrics Branch, the Publications and Reports Branch, and the Community Services Branch, all operating on an interdisciplinary basis.

The Training and Standards Branch of the Institute administers grants for improving and expanding graduate training in the fields of psychiatry, clinical psychology, psychiatric social work, and psychiatric nursing; stipends to trainees in the above four mental health disciplines; and grants to medical schools for undergraduate instruction in psychiatry. In the current fiscal year, $2,500,000 are available for graduate training grants and stipends and $400,000 for undergraduate psychiatric training in medical schools. Other training activities include demonstrations in professional education and conferences to improve teaching methods, such as the Boulder Conference on Graduate Education in Clinical Psychology held August 20 to September 3, 1949.

The research program of the National Institute of Mental Health is divided into research grants and intramural research projects. Grants-in-aid for research are made to universities and other public or private institutions and to individuals for research in mental health on the etiology, diagnosis, treatment, control, and prevention of mental diseases. Projects are supported from a wide range of disciplines and are not confined to any one field. In addition, fellowships are provided for the training of promising research workers, in any relevant discipline, who are interested in mental health. In this fiscal year, $800,000 will be available for research grants and $100,000 for research fellowships. Since this program has been in operation for three years and since many projects run from three to five years and many fellowships are renewed beyond the initial year, only a fraction of the above amounts will be available for new grants and fellowships during the year. The intramural research program is still in its initial stages of development and will entail a major interdisciplinary research program with its focal point at the Clinical Center now under construction at Bethesda, Maryland.

In addition to these research activities, the Biometrics Branch of the Institute compiles and evaluates statistics on patients in mental hospitals and other related problems, functions which were taken over from the Bureau of the Census in 1948. The Publications and Reports Branch is responsible for the planning, production and dissemination of information about mental health.

The program of the Community Services Branch is one of assistance to States in developing their own community mental health programs based upon local needs, resources, and problems. The expectation is that, with 53 States and Territories developing their own local mental health programs, the more successful patterns of outpatient, preventive, and positive mental health services will emerge for more widespread use throughout the country. In other words, this program is in the Public Health Service tradition, of encouraging the development of strong and independent local and State health resources rather than developing a large nationally integrated program and of emphasizing the control and prevention of disease rather than its treatment.

Prior to this assistance program, which began operation in July 1947, only 22 States had mental health programs. At present all but two of the 53 States and Territories are participating in the community services program. Grants to States available in this fiscal year total $3,550,000. The States are required to match one dollar of State and local money for each two dollars of Federal funds received and to submit a plan and budget

annually for the use of these funds. They may use funds for any mental health purpose they decide upon except institutional care. In the fiscal year ending June 30, 1949, the States budgeted 51 per cent for their mental health funds for the establishment, improvement, or expansion of mental health clinics; 15 per cent for preventive and educational activities; 11 per cent for professional services other than clinic services; 10 per cent for training mental health personnel; 11 per cent for central administration; and 2 per cent for special studies.

In addition to administering grants to States, the Community Services Branch provides professional and technical planning, development, coordination, and general direction in the assistance-to-States program. These activities are carried on through ten regional offices by 17 mental health consultants in psychiatry, clinical psychology, psychiatric social work, and mental health nursing. In so far as possible, each Regional Office is to be staffed with complete teams of mental health consultants in all four of these disciplines. In addition, the Branch operates and directs demonstration projects to try out new and better methods and techniques of rendering community mental health services for use by States in developing their local programs. The Branch also maintains liaison with other governmental and non-governmental agencies concerned with mental health problems, conducts comprehensive on-the-spot surveys of existing and projected State mental health facilities, and provides, upon request, consultative services to mental hospitals and institutions.

The mental health consultants in psychiatry, clinical psychology, psychiatric social work, and mental health nursing assigned to regional offices carry out the assistance-to-States program by maintaining liaison with State and local mental health authorities, professional groups, universities, and other institutions; by reviewing State mental health programs; and by recommending action on State mental health grant-in-aid plans and budgets. Their chief objective is to assist the States to develop community mental health programs which will: (1) determine mental health needs and resources as a basis for planning a comprehensive and adequate State mental health program; (2) establish an organization with responsibility and authority for the program; (3) develop public interest and responsibility for meeting the mental health needs of the people in the State; (4) furnish mental health information and encourage the application of available mental health knowledge by the public; (5) provide special opportunities for mental health education and training for physicians, teachers, nurses, social workers, clergymen, and others whose daily work may have a preventive and positive mental health influence on appreciable numbers of people; (6) provide educational services for the development of well-adjusted children and adults; (7) provide professional services to infants, children, and adults who have behavior, educational, vocational, personality, or social difficulties which interfere with their functioning to the limit of their abilities and with their personal and social adjustment in life; (8) support or conduct studies which will increase knowledge and make possible a better understanding and handling of behavior problems; and (9) increase the number and competence of psychiatrists, psychologists, psychiatric social workers, and mental health nurses qualified for work in community mental health programs by establishing and supporting service facilities in cooperation with training centers and by supporting the training of partially qualified mental health personnel.

The success of community mental health programs over the country will depend on the knowledge, methods, skills, and the quality and quantity of personnel the several mental health disciplines develop and bring to bear upon the mental health problems of diagnosis, treatment, prevention, and ways of promoting positive mental health. The mental health field is so large and the needs are so great that all the mental health disciplines will have to supplement each other's effort and extend themselves to the utmost if any real progress is made toward improving the mental health of the nation.

Consider the magnitude of the prob-

lem. Mental illness is prevalent in all communities, among children and adults and in every level of society. Moreover, it may be as malignant as a cancer or as mild as a cold. It is conservatively estimated that in the United States at least one person in twenty will be hospitalized for mental illness at sometime in his life and that at least one individual out of five will fall into some category of serious but less acute behavior disorders such as those often associated with criminality and delinquency, accident proneness, divorce, economic dependency, and other individual failures in the performance of major social and personal roles. The annual cost of handling behavior disorders and the loss in earning power and productivity in the communities of this country are calculated in billions of dollars, but the more important intangible costs in human misery and frustration, in disrupted homes, in children deprived of security, happiness and normal opportunities cannot be calculated. The urgency of this problem is further heightened by the forecast of an increase in behavior disorders out of proportion to the population increase unless something is done, because the incidence of mental disorders increase with age and the number of Americans aged 65 and over is expected to double in the period 1940 to 1975.

To meet these needs for community mental health services there are some 5,000 psychiatrists in the country, but probably not more than 2,000 engage in outpatient work, private practice, and other kinds of services at the community level. Of the 7,000 psychologists we have, there are probably not more than 1,000 in the community services field. Of approximately 1,200 psychiatric social workers, only around 800 work in community mental health. Of the 5,600 graduate nurses working in the mental health field, probably less than 50 are working in the community mental health field. Accordingly, in all the mental health disciplines there are probably not more than 3,800 individuals, including many working only part time, to serve the millions needing community mental health services.

As one of the mental health disciplines, psychology has unique and major opportunities and responsibilities for research, training, and service in the community mental health field that have scarcely been touched.

From the standpoint of psychological research, consider the present state of knowledge concerning the etiology, treatment, and prevention of mental illness and what we know about positive mental health or the principles which will insure mental health. The diagnosis and treatment of mental disorders at best is only in its early adolescence. Prevention is just a toddler and positive mental health is little more than a gleam in the eyes of the National Institute of Mental Health and a few individuals like Daniel Prescott and Ralph Ojeman with their mental health through education programs, Leo Bartemeier with his Cornelian Corners, Lloyd Rowland with his Pierre The Pelican for parents with their first baby and Edmund H. Bullis with his Human Relations in the Classroom. Our best developed community mental health resources are our clinics with their multidisciplinary or team approach and the various unidisciplinary services such as those rendered by psychologists, psychiatrists, and psychiatric social workers. Perhaps the most that can be said for these resources is that they are limited primarily to the early diagnosis and treatment of behavior disorders acute enough to be referred to them. Their work is preventive mainly in the sense that they attempt to prevent even more serious developments which might otherwise lead to institutionalization. Very few of these clinics and professional services attempt much in the way of prevention at the sub-clinical level and practically none do more than make occasional verbal gestures toward positive mental health, so great is the demand for all available clinical services and so great the lack of knowledge and skills in how to do preventive and positive mental health work. Unanswered are such questions as: What are the major kinds of mental ill health and in what segments of the population are they found? What are the factors underlying mental health and mental ill health? What specific mental health services are needed? How

should we go about meeting these needs? The answers to these questions will require research on the basic problems of behavior development; on the nature, etiology, and treatment of behavior disorders; on the development of mass screening and treatment techniques; on community life, its attitudes, cultural mores, problems, needs, resources, and potentialities; and on a host of other related problems, all of which are proper concerns of psychology in some degree.

Upon such researches as these will depend ultimately the answer to the question—what should be the preparation of clinical psychologists for work in the community mental health field? As we now know it, however, the demand is for well-qualified psychologists able to assume positions of professional responsibility and leadership and with enough knowledge of related professions and of the organization and dynamics of communities to recognize, use, and help develop potential mental health resources in the community. Beyond this, clinical psychologists working in this field, as in other fields, should be able to formulate a psychological diagnosis as a basis for treatment planning, participate in treatment planning, and carry out treatment plans either by providing consultation for others doing remediation or by executing direct treatment measures with consultation or supervision as may be needed. Training and experience for work with infants, children, and adults are also essential. In brief, the need in this field is for clinical psychologists who are interested primarily in extending the application of mental health principles involved in a given case to similar types of cases and in contributing as much as possible to positive mental health.

Concerning the need for clinical psychologists in the community mental health field, there is probably not a rural or urban community of 100,000 people in the country that is not in urgent need of a community clinic with at least one staff member from each of the four mental health disciplines, to say nothing of the needs for psychological services in schools, and other health, welfare, and social institutions. There is an immediate need in this field for a minimum of 1,500 clinical psychologists. Long-range estimates of needs far exceed these modest estimates and make community services potentially the largest field of employment for psychologists.

Study of data published recently by Wolfle and Black in the *American Psychologist* and unpublished data collected by Limburg reveals that of some 7,000 known psychologists in the United States there are probably only 2,000 in the clinical and other closely related fields of psychological services, with around 1,800 additional individuals in training. Between 900 and 1,000 of these 2,000 are estimated to be engaged in some type of community services work such as in clinics, schools, and other community organizations. Probably very few of those now being trained in clinical psychology are receiving adequate preparation or even orientation for work in the community services field. Whereas about 60% of the APA membership have completed their training for a doctorate in psychology, only about one-third of the psychologists now in the community services field have such training. As a group they receive the lowest salaries in the several occupational groups in psychology. Because of lower salaries and lesser status than other psychologists enjoy, psychologists in community services either have had less encouragement to complete their training or the field has not been able to attract better trained psychologists. This is a serious situation because outpatient work or work in open-ended settings like community services requires at least as high if not a higher degree of competence than is required in closed or institutional systems. For instance, in closed systems like hospitals, errors of omission or commission are more readily righted by supervisors than they are in open-ended or outpatient settings where patients are not under control. Also, the variety of patients and problem situations are greater in the community services field and require relatively more extensive if not more intensive skills. The need in this field is for better-qualified psychologists who can render more adequate community mental health services on a profes-

sional rather than a technical or clinically useful level. With more status and an improved salary situation, more and better psychologists should be attracted to this field. Needless to say, the National Institute of Mental Health is encouraging just these things in its assistance-to-States program and making every effort to break the impasse that has prevented greater contributions from clinical psychology to the community services field. While the Public Health Service has always been interested in mass services, it has never been interested in quantity at the expense of quality. Rather, in its long history it has found in comparable situations that the argument "something is better than nothing" may be a dangerous fallacy, especially if the "something," such as inadequate personnel, operates to spoil opportunities and make for failures. All too often, after such failures, areas have to lie fallow, sometimes for many years, before another attempt can be made.

In conclusion, the opinion is hazarded that if psychology supplies its share of the research and the personnel needed in the community mental health program, its success will have important implications for all fields of psychology. All psychologists are invited to consider their responsibilities and opportunities in the community services field, regardless of whether their principal interests be in systematic, experimental, animal, social, genetic, educational, clinical, or any other field of psychology. Contributions from all fields of psychology are needed and all psychologists will find some problem in the community mental health program worthy of their attention.

THE OPPORTUNITIES AND RESPONSIBILITIES OF CLINICAL PSYCHOLOGY IN THE FIELD OF COMMUNITY SERVICES

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The clinical psychologist working in the traditional setting of the clinic or hospital is accustomed to having his cases brought to him in his own office where he usually works with them on an individual basis. Feeling secure in his own setting, he recognizes the need for helping others to feel comfortable "about coming to see a psychologist." In community service work, on the other hand, the psychologist is frequently called upon to work in settings which are not familiar to him, and consequently needs to develop a comfortable feeling himself before he is able to help others feel at ease. In order to feel comfortable and to function effectively in physical health clinics, social agencies, schools and other community agencies, the psychologist needs to understand the structure of community organization. Instead of having people seek him out after problems have developed, he will have an opportunity to observe them in what approaches a normal situation.

Another way in which community work differs from work in the usual clinic setting is that the psychologist associates with different groups of workers. Most clinical psychologists have worked only with psychiatrists and social workers whose approach to problems is similar to theirs. In the field of community services, the psychologist works with members of many other professions; for example, the clinic nurse, the case worker, the teacher in the nursery, and the public school—all of whom have a background of training and experience quite different from his own. The fact that they may feel inadequate in meeting problems of social and

* Presented at the meetings of the American Psychological Association, Denver, Colorado, September 6, 1949, in the Division of Clinical and Abnormal Psychology's Symposium on Psychology in the Field of Community Services.

emotional adjustment, may make them feel threatened when the psychologist enters their domain. Although some psychologists have worked with all of these groups, in most instances, they have worked with them apart from the medical clinic, the social agency, or the classroom. When the psychologist works in their setting, rather than his own, he must build a cooperative working relationship which will help them to gain a better understanding of some of the mental health aspects of their work and to develop more effective methods of dealing with mental and emotional problems.

In regard to both the new types of situations and to the new groups of workers, it is suggested that the psychologist would do well to assume the role of "listener and learner." This will give the workers a feeling of security by making them realize that he is interested in learning more about their problems. It will also help him to gain a better understanding and appreciation of the situations that confront them. In reality, the psychologist will not be assuming a pose when he adopts this role, for if there is anything about which we can feel quite definitely, it is that we do not "know the answers" in this field. We have much to learn about helping people to meet problems of living in such a way that serious maladjustment, if not illness, will be prevented. Unless the psychologist or any other worker in the mental health field can help members of related professions feel comfortable in their mutual relationships, few constructive results can be expected. Workers in the mental health field undoubtedly have felt that they were expected either to know or to find out the answers. Consequently when they enter a new situation, they tend to give others the feeling that they will have all the answers. Not until we ourselves feel, and in turn help others feel, that we are working *with* them rather than *for* them, can we hope to gain acceptance.

Since other workers in the community often have rather vague concepts of the mental health specialties, they are apt to confuse their functions. If the psychologist is also confused, the situation will be all the more complicated. In the community service field, it is even more important than in the clinical field that the psychologist have an understanding of the special functions and contributions of the closely allied disciplines of psychiatry, psychiatric social work and mental health nursing.

Opportunities for the psychologist to do preventive work lie in three principal groups of community organizations — health agencies, public and private social welfare agencies, and educational institutions. Each community has a somewhat different type of organization, so situations will vary from one community to another, but the following are sufficiently typical to illustrate the preventive approach.

One of the best foci of attention is the pre-natal clinic. Many of the expectant mothers who come to a pre-natal clinic do not have resources for other medical and psychological assistance, and may not have read extensively about the normal growth and development of children. They are usually seen briefly by the public health nurse and/or clinic nurse and then wait for the clinic doctor. This waiting period provides a good opportunity to talk with them in small groups and to answer questions they may have about the coming of a new child. Classes or discussion groups organized especially for expectant parents afford other opportunities to the psychologist. Since these groups are made up of essentially normal individuals with normal problems, members of the group will frequently give the right answers to problems or situations which are discussed. This is often more effective than having the discussion leader attempt to tell them what to do.

In these discussions, one frequently sees evidence of a carry-over of problems from the parent's own childhood. There is also frequent expression of the extent to which a child is wanted or unwanted. Another normal concern is related to what should be or could be done if the child should be mentally or physically abnormal. One also encounters the fear of delivery. The father's attitude regarding the arrival of the new child is often discussed as is the problem of preparing older children for the coming of a new baby.

In one group in which the writer participated, this latter problem resulted in a rather lengthy discussion of sex education and methods of informing the older children about the coming of the new baby. Several mothers had expressed doubt as to how this could be done on a simple level. One mother said that she thought that one should try to tell children so they could understand. She said, "For my little folks about this high (about 30 in.), I tells them I got that new baby in a warm spot right close to my heart; and he is gonna stay there and grow until he is big enough to come out in this world of sunshine; and then he will play with you." Later she added, "I don't say he's just growing in my belly." Such a warm feeling expressed by this mother did much to illustrate to the group how simply and well an explanation could be given to a young child.

Another type of group situation which is usually provided by Public Health Departments is the well-baby clinic or conference. The mothers who attend are accustomed to preventive services in the physical field, and it is therefore relatively easy for them to see the value of discussions about normal growth and development. As might be expected, these mothers are only too eager to talk about the baby and the problems he presents. Through these discussions, they become aware that many of their problems are not unique, and in fact, occur quite regularly in a normal family life. The type of questions which arise can easily be imagined. Many questions relate to feeding problems—whether the child should be encouraged to continue breast-feeding, whether he should remain on a definite schedule. Habits of sleep, play and toilet-training are likewise discussed. Frequently, reference is made to the problem of sibling rivalry, and the young mother is often reassured by an older mother who has several children. Usually, it is not information which these people need or want, but rather an opportunity to express their feelings about what they consider their problems to be. The psychologist or the discussion leader must realize that he is dealing more with attitudes than with requests for knowledge and facts. The personnel with whom the psychologist works in such a setting are a public health nurse and a clinic doctor who are usually very busy with the mechanics of clinic routine. However, they are interested in problems of a psychological nature which children present and in learning more about how parents can be helped with these problems. In this situation, as well as in those which follow, there will be problems and obstacles in working in group settings with members of other professions. One has to try to objectify one's own feelings as well as those of the other workers.

Private and public welfare agencies represent another community area in which the psychologist might work. People frequently come to these agencies in time of need, and their problems are often of a psychological nature. For example, these agencies often deal with the very young child in connection with adoption. Here the psychologist can apply his knowledge of early child development in attempting to evaluate the potentialities of the child and to work with the agency in finding the "right" adoptive parents for a child with a certain type of personality. The entire area of foster home care, including the child's adjustment in the foster home situation, is one in which the social worker and the psychologist might work together.

Another group situation in which young children are involved is the nursery school. Although communities vary greatly in regard to nursery school resources, frequently one finds considerable limitations in the training of staffs. However, these people are usually eager for any in-service training program and welcome assistance with many of the problems nursery school children present. By going to the school rather than having individual cases referred to him, the psychologist has a better opportunity to observe the child in the group situation and to assist the teacher in understanding the particular behavior involved. He also has an excellent opportunity to discuss with the parents the problems of children in the early stages of development. As might be expected, many of these problems result from anxieties on the part of parents over the first

separation or from home or marital difficulties. By participating in the actual school situation, it is frequently possible to help the teacher understand the disturbing factors. Again the psychologist is more frequently dealing with feelings and attitudes than with the need for specific information relative to what action should be taken. Work with nursery school children provides an opportunity to deal with problems that might otherwise cause difficulty when the child enters public school. Hopefully, one may look forward to the time when public schools will be able to assume responsibility for the nursery age group.

A fourth area in which the psychologist can do community work is in the public or private school program. In fact, the work of the school psychologist is coming to be regarded as a field of its own. Although many school psychologists perform services which may be regarded as clinical in nature, there are many opportunities for work in the community services area if the psychologist can visualize them. One of the most important is the attempt to integrate the services of the school with those of the community in the field of health and welfare. The school psychologist, of all school personnel, is often in the best position to assist in the coordination of these fields since, in working with the families of school children, he frequently works with the school social worker, or visiting teacher. He can also be in a position to assist in the integration of school services, particularly in the fields of health, special education, and guidance. Another area in the community field is in connection with parent-teacher groups. Many PTA organizations have p r e - s c h o o l study groups, and all of them are interested in knowing about ways in which the home and school can have a better working relationship. Within the school community itself, the psychologist has an opportunity to work with teachers around their own problems as well as to help them with their feelings about youngsters who present particular problems. If the school psychologist also works in vocational guidance and placement, contacts will be made with business and industries in the community relative to the placement of pupils who complete or discontinue school.

There are other areas in which some clinical psychologists are working which have implications for a broader community program. Although many psychologists in mental hygiene clinics or child guidance centers may be concerned largely with the diagnosis and treatment of clinical cases, there is an increasing trend toward broadening the program in terms of community education. The psychologist working in the juvenile court setting also has an opportunity to help the community become aware of some of the unmet needs of children. Perhaps the use of psychologists in connection with the juvenile department of the city police department might reach these problems at an earlier stage. A greater use of group agencies in the community might also help children who show tendencies toward delinquency. In some communities, psychologists work as vocational counselors in connection with an organized community agency program. Although the stated purposes may be for vocational counseling, many personality problems and family situations present themselves. The psychologist may not treat these problems directly, but he has an excellent opportunity to refer them to agencies where more intensive help can be given. The clinical psychologist functioning in an industrial situation also has an important job in the broader community aspects of mental health as his work encompasses the worker's total living situation including his economic and social adjustment.

In summary, it is felt that the psychologist in the community services field must first evaluate his own conception of his functions and the possible contributions he may make. His training and experience have given him an understanding of basic needs of individuals and ways in which these needs may be met. Secondly, the psychologist needs to become more familiar with community resources including all types of agencies and organizations concerned with the health and welfare of people in all socio-economic levels. Only with a clear understanding of the functions of these agencies and the types of problems they encounter, can the psy-

chologist hope to be able to see how his particular specialty may be of value in meeting human needs. In addition, the psychologist must recognize the value of going to the working situation or setting of other workers instead of having cases brought to him.

Finally, the psychologist must come to know the methodology of allied fields and believe that by working with members of other professions new methods can be found which will help people live more normal and happier lives.

EXPERIMENT IN PSYCHOLOGICAL SERVICE FOR THE SMALL COMMUNITY*

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This report deals with an attempt to meet a practical need and to evaluate the effectiveness of the attempt. In the summer of 1948, a request was made of the Psychology Department of Northwestern University to suggest a program in child guidance for the city of Sterling, Illinois —a city of about 12,500 people. The request came from the Junior Chamber of Commerce. It seemed to the writer that this group of business men, with little or no preconceived notion of the nature of "child guidance" felt that a service of this nature for children in the community might be a worthy project for their sponsorship (in competition with groups such as Lions and Rotary, which currently supported projects for blind and crippled children, etc.). However, it seemed also that the group had a hazy idea of concepts such as "child guidance," "clinic," "mental hygiene," etc.

Subsequent correspondence and conference led to the following program: With Miss Beatrice Weiner,[1] the author spent three days in Sterling in February, 1949. Mornings of these three days were devoted to clinical interviews and examinations.[2] Afternoons were given to formal talks or informal seminar sessions with the following groups: mothers of preschool children, mothers of adolescents, grade school teachers, fathers, a combined teen-age and parent panel, and the combined PTA for the whole community.

Prior to the actual project, groups in the community—the county medical society, businessmen's groups other than the Junior Chamber of Commerce, the YMCA—were informed of the project by the sponsoring organization. Interestingly, the only reservation in enthusiasm at this stage came from the clergymen's organization. However, the guidance counselor[3] in the high school, a teacher who gave part time to this work, reserved her overt support until the first day of the program when she had opportunity to discuss the project with the writer. As the program developed, this teacher turned out to be of most valuable assistance in the program as a whole. An additional assistant[4] worked on a voluntary basis. The wife of a local physician, she was of considerable help in gathering case history material, and in implementing the mechanics of appointments, etc.

The "clinic" was publicized in the local daily newspaper in front page stories on four occasions: a week prior to the project, and each of its first three days. The final story carried a picture of the teen-age parent panel. It might be considered, then, that the project was given relatively good publicity.

Two months after the program was held

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1. U. S. Public Health Service Fellow in Clinical Psychology.

2. Scheduling of these clinical sessions was done by the Superintendent of Elementary Education, Mr. H. U. Challand.

3. Miss Mildred Krughoff.

4. Mrs. Peter Gray.

in February, a sampling of opinion was obtained through interviews of about 1 per cent of the population. This investigation was conducted by David Murray, whose study is reported in a separate publication.[3] In order to disguise the real intent of this investigation, questions largely of an open-end type were used. Dealing initially with what the respondent might do if faced with a problem in a child, questions then became more specific, in terms of what sort of service might seem important in relation to problem behavior in children, how such service should be supported, etc. Finally, the questions touched specifically on what the respondent knew of the program that had been conducted two months previously.

It is not the purpose of this report to present the details of this survey, but it was apparent that it yielded a double harvest. It was found that the program had reached a very small minority of the population, but also that there was an amazing dearth of real feeling for the concepts of child guidance or psychology or psychiatry, or the possibility of seeking help in relation to children's problems.

For example: in connection with *problems* in children (such as antisocial behavior, stealing, lying and excessive shyness) only three to four per cent of the population stated that they would seek outside help. However, in the case of failure in school, 50 per cent would seek such help. A very large proportion of the population would use physical punishment, 60 per cent for antisocial behavior, 40 per cent for stealing, and 40 per cent for lying.

Of those whom one would consult for child problems, teachers were most popular, with the physician in second place. But of *institutions* which are available, the church was in the lead.

Murray found that "psychologists were favorably considered by a decidedly larger number of people than considered them unfavorably. However, they were considered less favorably in general than school officials, close friends, and the YMCA; not to mention physicians, teachers, and the church."

In regard to the effect of the clinical program itself, the following results were obtained:

("Did you hear or read anything about the child guidance program held here in Sterling this past February? If so, what?")

71% No, hadn't read or heard about it.
12% Read about it, but don't remember what.
8% Heard about it, but don't remember what.
6% Attended one or more of the talks.
3% Heard about it and gave opinions on the program.

These figures indicate that despite widespread publicity, the particular type of program which was tried failed to register with many people. Impressions of the writer subjectively, and of those who were interviewed by Murray following the program, were that the *public lecture* aspect of the program was almost ineffectual in reaching any but a negligible minority of the population. It was impossible to determine how the actual clinical service (extended to, at most, fifteen families, on a confidential basis) might, if continued on a repeat-performance basis, finally achieve reputation with a substantial portion of the citizenry. It may be repeated that, when asked specifically, 70 per cent of the people in town felt the need of a permanent child guidance specialist or agency—to be supported preferably by town funds.

Summary

In this project, a "clinic" was held, for three days, in the town of Sterling, Illinois. The program, which was given what is felt to be relatively good newspaper publicity, consisted of several public lectures and seminars and a confidential psychological service to a few children and their parents.

Investigation of public opinion of 1 per cent of the town two months later showed that, in terms of their recall for it, the program (1) had reached a very small proportion of the people and (2) more important, perhaps, for our general understanding of mental health in the community, the stated opinions of these adults regarding their practice in dealing with children who might offer problems reveals

a marked divergence from what is generally considered hygienic and effective. Moreover, but a small proportion of the population would seek any help at all.

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PSYCHOLOGICAL RESEARCH IN THE COMMUNITY MENTAL HEALTH SERVICE FIELD*

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The need is obvious for the development of public health practices which will control the causes of mental ill health as effectively as is now done with many other diseases. It is equally clear that developing truly preventive community mental hygiene practices will be complicated and difficult, because the usual inhibitions to social action are confounded and multiplied by the threats created when emotions appear likely to become a subject for public scrutiny. Here is a rich area for psychological research opportunities.

It is the purpose of this paper to discuss the opportunities and the methodological problems for the psychologist doing research in the field of community mental hygiene. This discussion will be divided into three parts:

1. A description of some likely areas for research and some examples of research problems.
2. A brief listing of some criteria for choosing likely research emphases out of the wide range of possibilities.
3. An example of a hypothetical research program, described in order to show the problems and challenges in research methodology which exist in studying a community problem.

Research on community mental hygiene, it seems to me, can best be attacked by studying those factors in total community life which prevent or promote the development of healthy patterns of adjustment by individuals or groups. Stating this a little differently, research in community mental hygiene might best be concerned with the nature and the origin of conflicts, frustrations, or confusions for persons or groups which are part of the community way of life. It would also be concerned with those efforts in community life which are attempting to relieve the unhealthy influence of conflicts, frustrations and confusions. This means that research in community mental hygiene is work in an applied field which is intending to implement efforts in preventing mental illness on the community level.

In the light of this suggested direction it is possible to see four general areas of research. Let me mention the four of them and then give several illustrations of projects within each. The four are:

1. Research which can be done on mental hygiene activities which now exist in the community and which can be recognized as such, either in whole or in part.
2. Research which can be done on mental hygiene functions which do not now exist and for which there appears to be a need on the community level.
3. Research intending to make more clear the nature and sources of certain conflicts, frustrations, or confusions and of dynamics involved in efforts to remove these.
4. Research on the major sources and channels for inducing certain patterns of adjustment on the part of individuals in the community.

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I wish to express my thanks for the group consultation on this paper given by Drs. John French, Thomas Gordon, Ronald Lippitt, and Donald Nylen.

It may be helpful to cite several examples of research projects in each of these general areas.

Area One: Research Which Can Be Done On Mental Hygiene Activities Which Now Exist in the Community

One of the major difficulties in community mental hygiene lies in encouraging collaboration among those people in the community whose function directly aims at influencing the personality development of others. These include teachers, social workers, therapists, ministers, youth leaders, parents, and others. It is often true that people in these roles, in greater or less degree, misunderstand the abilities, responsibilities, and aims of one another and thus find it difficult to work together. It would be helpful, then, if we had data on hand which gave us some indication of the degree of misperceptions each of these roles has about the others in any specific community. A recent study by Jenkins and Lippitt[1] concerning the misperceptions that parents, teachers, and students have of each other illustrates very well the nature of the difficulty one meets in attempting to create collaboration on personality growth efforts among separate community roles. The next step in this direction, of course, would call for research on the most effective methods of removing these misunderstandings and setting the stage for collaboration activities.

Another related project is an attempt to determine the readiness which people with potential mental hygiene influence have for work in the community setting. Most of our thinking and training in mental hygiene is oriented toward the individual person. It seems feasible to hunch that the training of most of these potential prevention sources would make them unable and unwilling to think in terms of community factors which influence mental health. Such data would help us to predict the nature of the resistances we would meet as we attempt to change these persons into hygienists who are oriented toward working for community health.

A final example of a research activity on things as they now are is research in various methods of mental health education. It seems very unlikely that posters, newspaper stories, or radio talks will make much impact here. Study is needed on the best media for communication of such material but more important is the problem of the best conditions for receiving such information and making it a part of oneself. The State University of Iowa, for example, found that Mothers' Clubs who heard radio talks about child care as a group were much more influenced by the speaker than those who listened to the talk at home alone.

Area Two: Research On Community Mental Hygiene Functions Which Do Not Now Exist But For Which There Appears To Be A Need

Some of the most interesting research possibilities may be found in measuring the effect of introducing new practices in community mental hygiene. For example, many communities may be ready for the use of an at-the-elbow consultant for town administrative officials such as city councils, school boards, community councils, or others. A precedent for this kind of service may be found in the morale officer who advised the line-officers during the war on the psychological meaning of certain group behavior[9]. The work of Adam Curle[2], an English anthropologist, in consulting with the officials of a small town is another example of what we have in mind here. This suggests that there may be much to learn from instituting the role of the community group consultant who helps groups understand the nature of the tension releases they use as a group and the meaning that this may have for the health of individual members. The use of psychiatrists in consulting training groups at the National Training Laboratory in Group Development suggests that there is much value in a careful analysis of such a role among community groups[3].

There has been relatively little study of the effect of informal therapy groups, although the efforts of Bales[4] in examining the nature of Alcoholics Anonymous

indicates that groups which have no formal attachment to some institution can go far in helping people to understand themselves. A further study of the contributions of such informal therapy groups may be fruitful.

The dynamics of preventive work in mental health would also be enlightened by a careful study of the influence and role of community mental health aids such as used by Greco[5] at Aliquippa, Pennsylvania. Greco has enlisted the aid of a number of outstanding community citizens who can help his mildly-ill patients to meet some of their needs in the community. One helper may provide channels to group membership, another to certain skills, and still others to whatever may be seen as a way of meeting some of the citizen's needs.

A final suggestion on research studies of new mental hygiene activities is the heightening of community self-awareness among the town members so that their sense of town-belongingness is increased. It seems clear that communities with high transiency rates contribute much more than their share of mentally sick people. Although not all of the situational factors contributing to ill health may be relieved by raising each individual's sense of community belongingness, there is enough evidence from research in military, housing, and industrial morale to make this an area well worth further study. I would predict, for example, that a sufficiently heightened sense of community belongingness would lead to community problem-solving efforts which might go far toward relieving many of the conflicts, frustrations, and confusions present in a community, or to the development of community standards which would better define the roles and relationships between these roles of community members—leading to greater personal security.

Area Three: Research Intending To Make More Clear the Nature and Sources of Certain Conflicts, Frustrations, or Confusions in the Community Pattern

Several examples are all that need to be mentioned here. It is interesting that the well-developed methods for making morale surveys have been little used in the community. The experience of the bombing surveys[6, 7] in Germany and Japan can serve as examples of the type of work which would be useful to any program in community mental hygiene. The study made by Curle and Trist[8] in determining the adjustment problems shown by British soldiers after they had returned home from rehabilitation camps provides another example of the type of method which could be used in studying these problems.

Area Four: Research On the Major Sources and Channels for Inducing Certain Patterns of Adjustment Within Community Life

The selection of this research area is based on the assumption that many of the behavior patterns which are interpreted as symptoms of illness are adopted by contagion from those with whom one associates. It would be helpful, therefore, to know the contagion sources of these behavior patterns and the channels they follow through community groups.

For example, there seems promise in assuming that groups as groups use patterns of tension release which are adopted by the group members for handling their own tensions and which may or may not be healthy for the individual members, or for the group in the social structure. The psychiatrists, Main and Nyswander, in a recent unpublished study[3], report their perceptions that the usual group mechanisms of decision reaching, blackboard listing, talking off-the-point, intellectualizing, granting the floor to the strongest members, and many others have an important tension-releasing function just as do the more familiar practices of scapegoating, or hostile jocularity. The family, club, and work situations, then, may all suggest to the individual member certain patterns of adjustment useful to him. What these are and how they get communicated and supported is the research problem.

We have briefly glanced at a number of research possibilities in the general area of community mental hygiene. Obviously, there are many more problems

ready for attention from the psychologist in the community health service field. What criteria can one use for choosing a research emphasis out of the large array of possibilities? Let us look at a tentative list of criteria for helping in such a choice-making situation.

1. Since community mental hygiene is an applied field, it seems sound to assume that a major criterion is that the research be concerned with the engineering problems of implementing efforts to prevent mental illness.
2. The research will be most useful if it is concerned with ways of promoting mental health in groups or in the social structure rather than with individuals as individuals.
3. It will be better to concentrate on research concerned with distributing the responsibility for mental health over a wide number of persons rather than limiting it to the responsibility of the therapists and social workers, since community mental hygiene must affect the way of life of *all* members in that town.
4. It is best to start on problems which, when results are available, will show likelihood of influencing the largest number of people.

Even after wise selection of the research emphasis, there will be many difficult problems in the methodology of research in community mental hygiene. We can illustrate some of these by following through the hypothetical experience of a social psychologist as he develops a program of research.

When the story begins the psychologist is sitting in his office waiting for the appearance of a committee from a nearby town who have asked for research help. The town is an industrial one with a population of about 18,000. It has a history of attempts at fairly progressive programs in delinquency prevention through parent education and a noticeable lack of success in community integration shown by rivalry between organizations and apathy in community efforts.

As the professor muses about the committee's arrival he begins to speculate about the nature of the problems he will soon face. If he would soliloquize for us, we would hear him say:

"I am worried about how representative this committee will be. How will I get them to broaden their base of community representation? I wonder, too, just what will be my best role. They are coming to me as an expert. How can I get them to use me wisely and yet not hinder their own creativeness and involvement? They certainly must see the findings as *theirs* if they are to be able to implement them. What will be the best place to start? And should we begin with a pilot study, a diagnostic investigation, some field experiments, or what?"

His speculations are interrupted by the arrival of the committee. They are the editor, a school principal, and a family doctor. The discussion of this group leads to a later and larger meeting in the community made up of a more representative group. At the later meeting the decision is reached that the general problem is the apathy for, and resentment to, local mental hygiene activities. For the sake of economy it is proposed that a pilot study is needed which can be carried out by locally trained townspeople. The psychologist also sees this as an excellent means of widening the community involvement in this study. They determine to get some facts: on the readiness and resistances of local public officials and influential people for mental hygiene efforts; on the stereotypes of human nature and personality held by a random sample of the population; and on the nature of the educational activities among adults now carried on in the community which are related to their goals.

The psychologist pumps them for ideas and then prepares the interview schedules from the leads obtained from them. He trains the interviews and later develops the coding scheme. The data is gathered by the committee members and tallied one evening in the school gym at a tallying bee, with the help of training provided by the psychologist and some of his students.

The rather gross findings of this pilot study show that many people are ready to act in community mental hygiene efforts, especially the professional persons

who influence personalities, but that they do not know what to do. It is also clear that many uncoordinated mental hygiene activities are going on in the community and that most of these reach the women who are parents.

These and other findings are presented to a still larger group of people in the town. It is clear at this meeting that many of the key figures in town are against any attempts at mental hygiene efforts. They seem to think they are synonymous with "babying" people. They are able, however, to arrive at two further areas of attack:

1. A more careful diagnostic survey about what people in the town really believe and do about mental health. This is set up to be done by technically competent interviewers brought into the town.
2. A field experiment is begun in training certain key persons in the community in problems of human relations and how to deal with them. Measurements are developed for determining the effect of this program on the constituents influenced by these key persons. Since this is to be a demonstration project, very careful plans are laid for determining the means whereby this demonstration can be made most contagious to other comparable groups in the community.

In the meantime the psychologist has developed some crucial hypotheses of his own which can best be tested in the laboratory where it will be possible to better isolate his major variables. He is able to induce graduate students to work with laboratory groups on the nature of group tension relieving mechanisms, and the communication of misperceptions among group members.

When all these studies are completed, it becomes clear to the community that they are now ready for still further experimentation, this time with attempts to get feelings communicated to key community officials so that community actions consider feelings as well as facts. Thus, the psychologist finds himself ready to begin new field activities while looking toward the possibility of nailing down further crucial hypotheses in a laboratory setting at a later time.

This brief look at the experiences of a researcher shows that the methodology of community mental hygiene studies has within it special problems of involvement, the choice of the proper level of fact-finding, the most effective role of the researcher, the use of certain criteria for choosing a place to begin, and many others. Few areas for research offer so many methodological challenges for the psychologist with a social conscience.

The need is great for effective community mental hygiene. The number of research possibilities within this general field is large. It is difficult to conceive of a research area offering more promise for opportunities to increase our knowledge of psychology and to work at the same time on important problems within our society.

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THE TRAINING OF PSYCHOLOGISTS FOR THE COMMUNITY MENTAL HEALTH SERVICE FIELD*

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I

If psychology is to effect a substantial contribution to the field of community mental health services, it must take stock of what its productive functions are, can, or might be. It must also take stock of its trainees, its teaching staff, and its training programs with respect to effectiveness in meeting these current and potential functions. It must ask itself whether its turned-out product should be persons with a well balanced set of skills, or specialists with outstanding but restricted skills, or if its potential functions are so broad as to require many different types of skills and personalities. It must ask itself whether its training programs are perhaps over-committed to certain functions and under-committed to others, which, in long time terms, might make more effective contributions.

We shall begin this discussion by openly stating a bias in the following questions. Are we letting the obvious pressing needs of the mentally ill shape our training programs too exclusively in the direction of giving students knowledge and skills for diagnosing and for trying to pick up the psychopathological pieces? And thereby are we failing to train personnel adequately to meet the less obvious and probably more important needs of bending teaching, research, and clinical talents to the problems of prevention of mental ill health and to the problems of discovering and fostering the positive factors contributing to personal and social maturity, integrity, and effectiveness?

This paper is asking many more questions than it can answer or could answer with unlimited time, but it is hoped that they are questions upon which we will all ponder in the attempt to tease out leads to *some* of the answers.

The inspection of functions can be approached from many directions. Let us look at two. (1) The basic background and skills that psychology departments now give to students, which are of use to the field and which might be further exploited and extended. (2) The problems in the mental health field which need solution to which psychologists, as now trained or with modified training, could contribute.

II

The basic background and skills that training in psychology has to offer the field we shall briefly review under the headings of theory, research methods, basic knowledge, and specialized knowledge and skills.

Theory. As for theory, our students are inundated with all kinds, implicit and explicit, fragmentary and systematic, testable or a credo to be believed, molecular, molar, mechanistic, purposive, field forces, dynamic, constitutional, psycho-biological, psycho-social, etc. From this overlapping hodge podge some students emerge in semantic confusion, some with doctrinaire inflexibility; but the majority emerge with their own integration and are full of disciplined theoretical drive. To colleagues from other disciplines, they may appear fuller of theoretical cliches than substance, but for most students, this theoretical training means sharpened concepts, more clean-cut research, and greater clinical productivity. The usefulness of this submersion depends, of course, upon whether theory is competently taught so as to be regarded as a useful organizational tool productive of new research ideas or new clinical sensitivity, or whether it is an end in itself that produces aesthetic, albeit schizoid, satisfactions.

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Research Training. The psychology student has perhaps more disciplined training in regard to research design and methods than many collaborators in the mental health field. Therefore, he should perhaps be expected to make his larger contributions in research. This can be true, provided his research methods are for him again not an end but a means of organizing and discovering relationships among empirical data of theoretical relevance or practical importance to the field. If his training makes clear to him the objectives of scientific method, not merely equips him with some of its techniques, he will be able to modify his techniques or invent new ones to handle the multifactor, interacting variables with which research in this field must deal. He must have content knowledge of the field by first-hand contact with it if his research undertakings are to have relevance and substance. Certainly the need of relevant disciplined reality-oriented research in this bio-social-psychological frontier is acute. We have barely scratched the research surface with respect to the many facets of complex interrelationships, or even with respect to evaluation of many current research, diagnostic, and treatment procedures. We have practically not touched at all research upon prevention, or upon factors contributing positively to mental health. We must see to it that we do not permit the pressing service needs to decoy permanently too much of our research talent either by our training emphases or by disproportionate rewards.

Basic Psychological Knowledge. We all state that our students must have, in addition to research and conceptual tools, a background of basic psychological knowledge before they specialize. All would agree that sets of facts and principles which have the widest generality should be an essential part of a student's equipment. But much content is labeled "basic," the speaker suspects, because of tradition and not because of its proven generality outside of simple organisms in simple controlled situations. There is a tendency to dichotomize between basic and applied in psychology after the analogy of physics and engineering, depending upon the habitat of the investigator—if in the laboratory, "basic"; if in the field or clinic, "applied," which, of course, is nonsense. We cannot accept that the "laws of learning" derived from the media of nonsense syllables or from the elimination of errors by a rat in a maze have more than analogical generality to complex learning problems of human interpersonal relationships until research has established such generality. Many so-called "basic laws" just do not lend themselves to application, and many relationships of wide generality have been discovered by research in applied fields.

However, a broad sampling of the contemporary body of established knowledge in the whole field of psychology throughout its specialized areas should give an informed base upon which to build investigatory and service skills. But the student can be helped immeasurably to secure a broad and coherent base in this era of specialization and expansion if the staff consciously and conscientiously pursues an active program of integration. The process can be helped through integrative courses by staff members of wide perspective, by various devices of in-service training—*e.g.*, collaborative seminars, joint research projects, by a program which permits adequate time and opportunity for inter-communications among staff, and among staff and students, etc. Such programs can not only give the students a broader and more coherent base but the faculty as well.

From the applied areas our student obtains knowledge of more immediate relevancy, and attains skills with tools more immediately responsive to the problems of the mental health field. The contributions of sampling and polling can aid in surveys of common problems in need of attack. The tools of testing, both objective and projective, interview techniques, and group dynamic methods can have a fairly direct usefulness both in research and in the service aspects of diagnosis and treatment. Our danger here is that we may over-stress the immediately useful rather than follow leads that eventually might prove more productive.

Another approach to inspecting appro-

priate or effective functions to which psychologists might contribute or be trained to contribute is that of the needs still unmet in the mental health service field. They are legion. Let us look at the training opportunities of just a few of the established community agencies which inevitably meet psychological problems both of a staff morale nature and among their clients, and thus are involved either implicitly or explicitly with mental health or its lack.

1. *Educational agencies* from preschool through college present largely uninvestigated and unsolved problems of academic and social learning, of motivation, morale, status, aspiration strains, special problems of deficiency, and authority problems—inextricably mixed with problems of inter-personal relationships of teachers, administrators, school boards, and parents.

Psychologists, of course, have participated in schools with many problems but have failed, in the speaker's opinion, to take advantage of its large arrays of normal, happy, effective children for studying the factors which contribute positively to mental health. Under the never-ending pressure of need for helping deviants, we have spent too little time upon the healthy group from whom, in conjunction with our knowledge of deviants, we could learn what the real differentials *are* which make for maturity and zest or their lack. As psychologists, we have contributed little to the field of experimental education with respect to preparation for parenthood and family living, mental hygiene, teacher morale, group dynamics, and we have even failed to sharpen student awareness of and interest in the many unsolved problems which profitably could be attacked.

2. *Health agencies* offer myriad possibilities for psychological functions. The prenatal clinics, obstetrical wards, and well-baby clinics offer a rich array of opportunities for psychological investigation and service with respect to the surrounding strains and supports in which the newborn begins his trek toward mental health or ill health. This is one of the high strategic age areas for effective attack upon the problems of prevention which has been largely overlooked in the training programs.

Agencies which handle dependents either through institutional or foster home care again offer rich opportunities for psychological services. The study of psychological atmosphere in institutions, of group dynamics, and of methods of management could shed light upon an area heavy with mental hygiene implications. The field of foster home placement, with its meshing or failure to mesh the needs of both the child and the foster parents, offers a field which could be rewarding both with respect to research possibilities and with respect to individual help. And yet very few training schools have adequately exploited these possibilities.

With agencies frankly committed to mental hygiene functions such as counselling centers, psychiatric clinics, and hospitals, we see our possibilities more clearly and are contributing heavily, and our contributions will be greater, of course, as our expanding training programs improve. We are well aware, from the Mental Health Act provisions and from the Veterans Administration and state programs, of our responsibilities in contributing to the training of more well balanced, effective personnel needed to man the increasing numbers of such agencies. But all too frequently we have overlooked the psychologists' opportunities for contributions to the community services programs in agencies widespread in their implicit mental health functions. The speaker feels strongly that we will never make a dent in reducing the number of people who need intensive expensive individual care *unless* we tackle the whole problem broadly, at the early ages, and with a strong preventive bias, which we must get into our training programs at both the service and research levels.

III

Training for Community Mental Health Service

Let us inspect now three aspects of adequate training for present and potential functions, namely, trainees, training staffs, and programs.

The selection of suitable trainees is an area in which we need to engage in both straight thinking and adequate research. Are the requirements the same for personnel who will go into university teaching, into research, into activities which involve effective relationships with agency personnel, and into areas of work with disturbed persons? Can we, with realism, expect from candidates all of the requisites simultaneously for all of the divergent functions which are important to the broad field?

We all know, among our former teachers and present colleagues, brilliant and productive research workers with little interest or talent in teaching. From their singlemindedness of purpose, their devotion to research ideas, and their compulsive commitment to pushing research through, come important contributions. But you can imagine what they would do to themselves and to troubled patients if their compulsive and orderly personalities got mixed up in clinical service functions. All of us know talented and informed teachers who have made tremendous contributions to the field through their stimulating impact upon students, and yet have done minimal research. All of us know psychologists, mature and wise, whose libido and talents lie in personal relationships and service activities with persons and who contribute importantly to the field of training, and yet for whom hair-splitting abstractions and mathematical symbols are irrelevant or panic-producing.

In our selection of students, can we find enough paragons capable of meeting all these functions, and could we survive them if we did? Rather, from our potential pool of talent must we not seek combinations of different selective criteria for different functions, if we are to avoid losing trainees of high potentials for many functions which probably demand different combinations of abilities and temperament?

In our present phase of selection, we are more clear as to negative aspects as shown in our elimination of those we judge without ability to meet our present academic hurdles, and with respect to those entering clinical specialization in our elimination of those too hostile, punitive, narcissistic, compulsive, or power-driven. It is not just the applicants who have bearing upon our selection criteria. These criteria must also be geared to the specific training staff with its unique arrays of talents and value systems.

Training Staff. It is obvious that we need, as training staff, persons who satisfy the criteria we demand of our students. That is, we need a staff with a variety of talents, skills, and points of view, who likewise are not narcissistic, punitive, power-driven, or doctrinaire if they are to furnish an atmosphere evocative of student growth, independence, and integrity. We need teachers who derive genuine satisfaction from watching students mature and achieve competences greater than their own. In short, we need a stimulating, supportive, and mentally hygienic staff if we are to induce mature, zestful, and competent personnel in the mental health field. This is not an easy achievement in a restive transitional period.

With an increase of students, too sizeable and too rapid for facilities and staff; with the competitive demand for personnel far greater than supply among universities, research, and service centers; with the shifts of interest, emphases, prestige, and monetary rewards within the field; with training staff over-extended and/or threatened by these shifts, it is a challenging period but one of strains and confusions. But if we teach within our competences, limit our students to the number we can handle adequately, seek to be mutually supporting, perhaps we can hang on until relief arrives. The relief which is on its way from our graduate schools looks very good indeed, for by and large, the present group is more mature and experienced than the average, better selected, and has had better training than their teachers have. And that is the compensation which far outweighs the present strains or doubts.

Training Programs. Time precludes discussion of details of training programs for community mental health services. Only a few scattered comments can be made.

As yet, with our variety of teaching personnel and budgets, standard programs

comparable to those of professional schools cannot now be achieved, and we may never find it desirable or possible. As departments, we must each survey our own talents, supplement them where we can, but stay within the training functions which are appropriate to these talents. Better that we maintain substantial standards with fewer training objectives and fewer students than thinner standards with wider coverage. In the speaker's opinion, it might have been wiser in the clinical field, until we had more experienced staff available, had experienced clinical teachers been concentrated in fewer centers so that each of these centers could have done a more effective job, instead of spreading them thinly over a large number of institutions.

We must face, too, a reasonable decision with respect to service needs for persons trained below the doctorate. It is clear that there are useful activities to be performed in civil service positions already established for whom there will never be enough doctorates. In state universities charged with obligations for state welfare, we must face responsibility for training for these needed services, either directly or through seeing that training is adequately covered by other state educational organizations.

We need periodically to review, as a staff, our courses to the end of reducing ineffective overlap, of checking the perpetration of vested-interest teaching habits and course content, and for increasing the relevance of methods courses to shifted field interests. We should review the experience we offer for research maturity, for acquiring teaching skills and skills relevant to a variety of special field problems.

We should review the details of student load, morale, and the requirements for fostering the students' greater talents. Especially in training in the clinical field do we need to be alert to timing and sequences of experience which will facilitate, not retard, maturity. For example, students should not be dumped into adult psychopathology before having had professional experience with younger, more normal persons, or persons with milder problems. Students in the clinical field need a wide array of experience, but so arranged as to insure their development and not the activation of anxiety past the point of profit.

With the speaker's early stated bias for greater sensitization of students to attacking the problems of preventive mental hygiene and to the study of effective persons, this training, coming early in the training sequence, could help in handling the matter of graded doses of maturing experience.

Conclusion

The present shift toward training more psychologists to investigate, to understand, and to give service to the field outside the laboratory has raised and will continue to raise training problems. The approaches to solutions of the problems entailed in this shift with respect to optimal balances of trainee interest, staff, and program details, will take time, experimentation, and patience if training for productive functions is to be achieved according to priorities essential to maintaining these functions. If we build openmindedly, teach what we can teach well, and try to extend our competences at a reasonable tempo, we are on safe ground. If we do not let ourselves be stampeded by a feeling of neurotic urgency, or frozen into neurotic resistance in the face of demands greater than we can now meet, we should maintain enough flexibility and security to contribute substantially to our own discipline, and thereby to the mental health field. For in spite of strains, excesses, deficiencies, and confusion in our training programs, the field is full of enthusiastic, industrious, and productive graduate students, for whom we take some credit and from whom we expect help in improving training programs.

A SURVEY EVALUATION OF THE KINSEY REPORT

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Introduction

Not since 1859, when Darwin published *The Origin of Species,* has a publication in the field of science caused so much discussion as has arisen from the publication of the Kinsey, Pomeroy, and Martin report, *Sexual Behavior in the Human Male*[3]. Comments of scholars and scientists concerning the significance of this report reveal a wide diversity of opinion. Evaluations range from enthusiastic endorsements of the whole project to those which consider the entire research as worthless. Lack of agreement should serve as a challenge for closer scrutiny of the whole project and as a stimulus for a more thorough evaluation of the significance of the research.

One group of criticisms consists of those which more or less constitute a wholesale disapproval of the entire Kinsey research project. In order to give the reader examples of such criticism, three are here reported. Dr. Harold W. Dodds, president of Princeton University, has stated, "So far as my reading has gone, the recent sex-survey reports seem to be acclaimed more by the literary reviewers than by the scientists. We laymen (*yet speaking ex cathedra as President of Princeton University**) shall be wise if we pay serious attention to the scientists who question the methodology of the reports and the significance of their findings. Perhaps the undergraduate newspaper that likened the reports to the work of small boys writing dirty words on fences touched a more profound scientific truth than is revealed in the surfeit of rather trivial graphs with which the reports are loaded."[1]

A similar criticism is made by Dr. Geoffrey Gorer, a cultural anthropologist, who states: "It is greatly to be regretted that a study on which so much time and money has already been spent, and which contemplates the spending of considerably greater amounts of both should be so inadequate scientifically."[2]

* Italicized words inserted by author.

Dr. Margaret Mead, also an anthropologist, attacks the report as offering society "nothing to build with." "The Kinsey report is not daring. It does not talk about emotion of sex, its meaning to people. . . . We need to think more about sex as a part of inter-personal relations." She criticises Kinsey for handling the subject of sex "as an impersonal, meaningless act" and for perpetuating the Puritan attitude prevalent in this nation, and calls this attitude "extraordinarily destructive of inter-psychic and inter-personal relationships"[4, 5].

Such criticisms as given by Dodds, Gorer, and Mead not only constitute a professional challenge to the entire Kinsey research staff, but also are directed against the scores of recognized scientists who have served as consultants to the research project and who therefore are in some degree responsible for its general structure and development. In a less direct way, these criticisms are also directed against the various institutions, agencies, and foundations which have formally recognized the whole project, and which have given in one way or another their active support to the study. One of these is Indiana University, which has recognized for the past ten years the Kinsey project, and which has allocated money, personnel, and space for its support. Also involved is the National Research Council, Committee for Research on Problems of Sex, which gave its formal endorsement of the study in 1942, and since then has assisted materially in its financial support, largely through funds granted by the Rockefeller Foundation. Therefore the validity of such wholesale disapprovals of the study raises a serious issue.

The scholar should be equally cautious of blind and enthusiastic endorsements of an entire study or project. Usually scientists and scholars do not publish their approval or acceptance of research data. They merely incorporate into their work and studies the findings which appear

valid and useful to them. If they find a basis for significant criticisms, then they are more likely to announce their evaluations publicly. Such criticisms often contribute to the refinement and advancement of science. Instead of wholesale approval or disapproval these types of criticisms usually refer to such elements of the study as methodological procedures, inconsistencies in the published data, neglect of integral elements in the research design, limitations set by the sample or population involved, and interpretations not warranted by the data. The right of free and open evaluations of research is necessary for the advancement of science. Certainly no scientific endeavor is beyond criticism. Any scholar is entitled to render an evaluation of a study, but his status as a scholar depends upon the validity of his evaluations. When an individual incorporates his own biases or moral judgments in the evaluation of a research study, then the question must be raised as to the objective nature of such evaluations.

An example of possible moralistic involvement in evaluation appears in Terman's concluding statement in his review of the report: "Nevertheless, the passages quoted are just the ones that are most likely to impress youth who is in search of authoritative justification for the unrestrained satisfaction of his sexual urges"[6]. This statement apparently is based more upon moralistic concern rather than upon scientific evidence, since Terman offers no substantiating data for the claimed effect of the Kinsey data. The writer of this paper feels that systematic studies are needed which would establish the effects that this or any other book have upon behavioral patterns. The effects ascribed to the Kinsey report by Terman must be regarded as speculative until adequate data support his viewpoint.

The Study

Since individual scholars such as Dodds, Mead and Gorer have individually rendered a general disapproval of the entire Kinsey research project, it was felt that a more comprehensive evaluation of the over-all significance of the project might be of value. In order to obtain such an evaluation, a questionnaire ballot was sent to *all* Fellows of the Division of Clinical and Abnormal Psychology of the American Psychological Association. The group numbered 330 Fellows according to the *1948 Directory, American Psychological Association*[7]. Of the 330 Fellows, 229 are men, and 101 are women. Nearly every Fellow of the Division of Clinical and Abnormal Psychology holds a doctor's degree in psychology, and has had at least four years of experience in clinical and abnormal psychology. Thus it was felt that in general most of these individuals were qualified to render at least an over-all evaluation or opinion concerning the significance of the Kinsey report.

A ballot was sent by mail to each member during the summer months of 1948. The 295 who returned ballots constitute 90 per cent of the possible number of respondents. The number of unreturned ballots from males was 8 per cent and from females 16 per cent. The return of 90 per cent of all ballots is sufficiently complete to warrant fairly valid generalizations for the entire population covered by this study.

The specific question submitted to the group was: *"Do you or do you not believe that the Kinsey research project is sufficiently worthwhile to be continued to completion?"** Two response categories were provided on the ballot: "Yes" (I do believe) and "No" (I do not believe). Each respondent was asked to indicate his evaluation by checking one of the two categories. Following the response categories was the statement, "Comment if you wish," and space was provided for the entry of any comments. The "Yes qualified" category was created for the six respondents who checked the "Yes" response and who then modified the category by adding a word or phrase. The classification of checked responses to this question is presented in Table 1.

The evidence shown in Table 1 indicates that approximately 95 per cent ("Yes" and "Yes Qualified" responses) of the Fellows in the Division of Ab-

* Dr. Leo Crespi assisted in the formulation of the questions in this survey and in the preparation of the covering letter.

TABLE 1. *Responses to the question "Do you or do you not believe that the Kinsey research project is sufficiently worthwhile to be continued to completion?"* (N = 295)

Responses	*Male*		*Female*		*Total*	
	N	*%*	*N*	*%*	*N*	*%*
Yes (I do believe)	200	95	73	87	273	93
Yes, qualified	4	2	2	2	6	2
No (I do not believe)	2	1	5	6	7	2
Comments only	1	..	1	1	2	1
Not qualified to answer	4	2	3	4	7	2
Totals	211	100	84	100	295	100

normal and Clinical Psychology of the *American Psychological Association* considers the Kinsey research sufficiently worthwhile to be continued to completion. Analysis of these responses according to sex of the respondent reveals that 95 per cent of the males gave an approval for the continuation of the research and that 87 per cent of the females gave a similar evaluation. Only 1 per cent of the males gave an over-all disapproval of the Kinsey research project, while 6 per cent of the females disapproved. Thus 95 per cent of the psychologists included in this study have rendered an evaluation of the Kinsey report which is exactly opposite to that given by Dodds and others previously mentioned. Only 2 per cent of

TABLE 2. *Classification of 156 comments*

	Percentages (N = 156)	
I. Comments added after a "yes" or "yes qualified" response.		
A. General approval		
1. Significant, worthwhile, needed study	32%	
2. Value to psychologists, psychiatrists, etc.	7	
Sub-total		39%
B. Yes, but opinion based on reviews, not original report		2
C. Contributions far outweigh criticisms		5
D. Emphasis made on the necessity of freedom of research in human sexual behavior		6
E. Comments on social implications of the data		4
F. Criticisms of methodology		
1. General	4%	
2. Sampling and statistical	8	
3. Interviewing techniques	4	
4. More emphasis on "psychological" aspects of sex	5	
5. Interpretations and generalizations	2	
6. Addition of specialists to the staff	4	
7. Staff should profit from criticisms	3	
8. Publicity criticised	3	
Sub-total		33%
G. Yes, but not an important study		1
		90%
II. Comments added after a "no" response. General disapproval of the study		4
III. No response checked but comments given		6
Total		100%

all the respondents were in essential agreement with President Dodds' statement[1].

The reader should not interpret the 95 per cent ("Yes" and "Yes Qualified") over-all approval figure as meaning that these respondents have given a complete approval of each and every aspect of the study. Undoubtedly most of these respondents could make some constructive criticism or suggestion for the refinement and improvement of this study. In fact, approximately 14 per cent of all respondents offered some specific criticism or comment after giving an endorsement for the continuation of the study. Some of these criticisms will be presented in a following section.

There were 156 comments added by the 295 respondents under the heading "Comment if you wish." Of these comments, 141 followed the 279 "Yes" or "Yes Qualified" responses; 6 followed the 7 "No" responses; and 9 were added when neither response category was checked ("No response" or "Not Qualified to Answer"). These comments were classified according to the nature of the comments or criticisms, and the results are presented in Table 2.

Representative comments placed into each of the classifications shown in Table 2 will be given. In Section I of this table are listed various classes of comments which followed a "Yes" or "Yes Qualified" response. The percentages shown are based upon the 156 comments added by the respondents. Into the group which rendered a general approval of the report fell 39 per cent of the comments. These are placed in Section "I, A." Examples of such comments are presented.

I,A. General approval.

Case 57. "It represents an attempt to gather data in a scientifically acceptable manner on a subject which has been surrounded by more fear, prejudice, and misinformation than almost any other subject. Accordingly it should be supported and encouraged to the utmost."

Case 176. "Definitely. The Kinsey report is unique in my experience in that it deals with sex as objectively as though it were a treatise on the internal combustion engine. I think it is important because we need to know as much as possible about human beings and this aspect has been too long neglected, at least so far as so called 'normal' persons are concerned."

Case 290. "This is the type of study which promotes growth toward maturity of the social group, since it reduces illusions and stimulates a realistic approach to an important problem."

Case 221. "It is sound scientifically, objective and most worth-while."

Case 259. "About as important as any scientific study of man so far executed."

Case 122. "It would be disastrous for science and especially for psychology to discontinue Kinsey's work."

Case 198. "Enthusiastically. I know of no research that is more important for human welfare. Kinsey has done the best job so far on this topic. Kinsey has shown the highest ideals as a scientist and has clearly indicated the limitations of the study."

Case 168. "A monumental work. One of the most important studies of this century."

Case 20. "No more important research in any field is, to the best of my knowledge, being conducted in the world today. Nothing should be allowed to interfere with its completion."

Case 88. "The work is invaluable in training clinical psychologists and psychiatrists."

I,B. Yes, but opinion based on reviews, not original report.

Case 71. "I have not read the report, but have read reviews of competent scientists."

In Section "I,C" of Table 2 are listed those comments given by respondents who consider that the contributions of the Kinsey data far outweigh published criticisms concerning it. The following quotations are examples of such comments.

I,C. Contributions far outweigh criticisms.

Case 220. "Improvements in the techniques used might be suggested by some, but the inadequacies mentioned by the critics ought not to detract from the positive values of the data already accumulated."

In the next Section "I,D" of Table 2 are classified those comments which emphasize the scientist's right to conduct research in the area of human sexual behavior. The following statements are characteristic of the responses classified under this heading.

I,D. Emphasis made on the need for research in human sexual behavior.

Case 45. "I consider this study a fundamental contribution done by a group well-qualified as scientists. If social science research is not to be made the butt of all jests—this study deserves the opportunity to continue its fact finding procedures until the evidence gained satisfies the basic requisite of a sound philosophy of science."

Case 105. "There is certainly a tremendous amount of more accurate information than we ever had before. Any question of social significance on which at least some facts can supplant myths should be investigated. There is so much heat apparently about this book that it should be emphasized that honest inquiry into anything is worthwhile. I cannot see that science or religion or politics has anything to fear from truth."

Case 239. "The Kinsey research project seeks to provide factual material in a field where traditions and opinions hold us in bondage. We need much more data and the continuation of the Kinsey research project promises to supply us with data, factual material and increased awareness in one of the most important aspects of human life and behavior."

In Section "I,E" of Table 2 there are placed those responses which consider certain social and moral implications of the Kinsey data. Two comments from this group are listed below.

I,E. Comments on social implication of the data.

Case 143. "Control aspects are directly involved in most serious desiring for knowledge of human sex behavior. Yet to reject, ignore, or evade this involvement is probably very smart and expedient in our culture. Hence this study seems low in critical acumen related to formulating the crucial problem and in social responsibility for developing interpretations of data for those who wish to escape or enforce some form or degree of control. The solution, in my opinion, is not to restrict but to extend and supplement the project, especially to include direct acceptance of normative and evaluative problems, and, since controls will be directed towards producers of sex patterns—not their products—to problems of the structure of sex attitudes and sex personality types."

Case 118. "On the grounds that factual information is always desirable. The big question will be what our culture does about it. I must reserve the right to say that 'what is' and 'what ought' are seldom the same. I seriously question the possibility of scientific ethics, which Cattell has recently defended."

In Section "I,F," "Criticisms of methodology," of Table 2 are placed those general and specific criticisms given by individuals who have indicated their approval of the Kinsey research project and then offered a criticism or suggestion for future consideration. This group of criticisms constitutes 33 per cent of all comments. In such a tremendous and comprehensive scientific investigation as that being undertaken by the Kinsey staff, there is no doubt that specialists from many fields can contribute to the refinement of such a study. The complexity of such an investigation of human behavior makes it necessary for the investigators to rely heavily upon experts in areas involved in the investigation. Kinsey states in his first progress report that scores of specialists from many areas have rendered invaluable assistance to the staff in the development of the research project. The comments given below by the group of psychologists surveyed range from very valid criticisms to those which reflect merely the emotional reactions of respondents. Criticisms of methodology are classified in Table 2 under I,F, and examples follow.

I,F,1. General criticisms.

Case 253. "There is a tremendous amount of 'Information' in this study but it lacks something. It is my judgment that it lacks true sophistication in its methodological approach. It also lacks a cool perspective in its analysis and syntheses. To those of us who have tried to read the literature from the old masters, such as Havelock Ellis on women, the study leaves us with a feeling that something is absent that should be there. In spite of this I believe the study should be continued but consideration given to pertinent criticisms."

In Section "I,F,2" are classified comments which refer to sampling and statistical aspects of the study. For example,

I,F,2. Criticisms of sampling and statistics.

Case 242. "I think that the criticism that medians should be used is valid."

Case 74. "It is desirable that all statistical controls and sampling problems be improved so as to eliminate spurious interpretations based on insufficient evidence or biased groupings."

In Section "I,F,3" of Table 2 there have been classified all the criticisms referring to interviewing techniques involved in the study.

I,F,3. Criticism of interviewing techniques.

Case 132. "Believe direct question method often superficial but it is a first step in discovering factors in human sexuality. By all means the study should be completed."

In Section "I,F,4" of Table 2 there are grouped those statements which deal with the so-called psychological shortcomings in the present report. The general opinion expressed in these comments is that more emphasis should be placed on

psychological and emotional aspects than have been included in the report. Examples of such comments are listed.

I,F,4. More emphasis on "psychological" aspects of sex.

Case 127. "There is no question in my thinking regarding the value of the study. I feel that much more work of a psychological nature needs to be done but this taxonomic study will undoubtedly make such studies easier to accomplish."

In Section "I,F,5" of Table 2 are placed criticisms dealing with interpretations and generalizations. An example of this type of criticism is given below.

I,F,5. Criticisms of interpretations and generalizations.

Case 133. "Both of the above (responses) are with the proviso that Kinsey, or someone else, should more explicitly restrict the scope of his generalizations so that they are in closer accord with his data. The data are facts and should be public."

In the next section of Table 2, "I,F,6" are placed all comments which suggest that in the future Kinsey should add certain specialists to his research staff.

I,F,6. Addition of specialists to the staff.

Case 142. "I think it desirable that consultants from other fields (psychoanalysis, psychiatry, psychology, social work, anthropology, education, etc.) be invited to assist with the project."

In Section "I,F,7" of Table 2 are classified all statements which suggest that Kinsey should consider the criticisms which have emerged since the publication of his report and incorporate those which are valid.

I,F,7. Staff should profit by criticisms.

Case 188. "In the first place, I believe that the Kinsey Report is of sufficient importance that it should be carried on to completion more or less along the lines envisaged by Dr. Kinsey. He should report on his findings for women and then should continue his studies so that he has adequate sampling for both men and women. I am sure that Dr. Kinsey should take to heart the many criticisms that have been made of his sampling procedure and modify his plans to meet these criticisms. I also believe that Dr. Kinsey should be sensitive to criticisms of his method and, if necessary, modify his procedure so that there will be more general conviction that his results faithfully report the situation."

In Section "I,F,8" of Table 2 were placed all of the statements which constitute a criticism of the publicity which accompanied the release of the Kinsey Report.

I,F,8. Publicity criticized.

Case 162. "Might have been continued more objectively if there had been an attempt to avoid the type of publicity it has had and dramatization of total research—I would question effect of publicity on possibility of keeping method satisfactorily scientific."

In Section II of Table 2 were classified all the comments which were made after a "No" response to the question being considered. These responses constituted 4 per cent of all comments recorded. All individuals who made a "No" response added a comment.

II. General disapproval of the study.

Case 38. "Decidedly no. It seems entirely too narrowly conceived. Kinsey is not a psychologist and I am not sure he is conscious of the full nature of his problem psychologically, educationally, or ethically."

Case 174. "I regret to have to say that I do not think the Kinsey type of study is very worthwhile continuing, and certainly not with a zoologist in charge of the project. There are anatomical, physiological, neurological, etc., activities involved, and these continue to be investigated by competent people. There is also the cultural factor, and various anthropological considerations. The psychological, psychoanalytic, and abnormal aspects appear also to be important. Perhaps the most important aspects are the cultural, moral, ethical, religious, economic, etc. This particular 'drive' is very different from all others, for many reasons; and such a study should be conducted by a much broader type of scholar, one who has an intellectual appreciation for other fields of knowledge. Kinsey's procedures are faulty, and so are his statistics, etc."

Case 171. "Because I think censorship is unwise and do not care to have more of such material put into the hands of young people, I would like to see the research project ended."

In Section III of Table 2 were placed those comments given by individuals who did not check either of the offered response categories. Six per cent of all the responses fell into this classification. Most of the comments were statements to the effect that the individual had not read the report or did not want to make a decision in the matter.

Table 3 presents an analysis of the various classified responses according to the age of the respondent. The ages were obtained or estimated from the personal

TABLE 3. *Age of respondents and nature of the responses given to the question, "Do you or do you not believe the Kinsey research project is sufficiently worthwhile to be continued to completion?"* (N = 295)

Age ranges (year of birth)	*Responses*								*Not qualified to answer*		*Total no. of respondents*
	Yes (I do believe)		*Yes, qualified*		*No (I do not believe)*		*Comment only*				
1870-79	0		0		0		0		1	100%	1
1880-89	16	73%	2	9%	1	5%	0		3	14	22
1890-99	61	90	1	1	3	4	0		3	4	68
1900-09	127	96	1	1	3	2	1	1%	0		132
1910-19	60	97	1	2	0		1	2	0		62
Age not available	9	90	1	10	0		0		0		10

data given in the *Directory 1948, American Psychological Association*. For ten of the number who returned the questionnaires no data was given on which age could be estimated, and therefore their responses are classified under the heading "Age not available."

The data in Table 3 concerning age and nature of the responses reveal a fairly definite pattern. Of the respondents born after 1900, 97.4 per cent considered the Kinsey research project sufficiently worthwhile to approve its continuation to completion. Of those born before 1900, only 88 per cent of the respondents gave a "Yes" response to the survey question. Of the remaining 12 per cent in this latter group, 4.3 per cent gave a "No" response, while 7.7 per cent did not respond to the question by either "Yes" or "No." Most of this 7.7 per cent group stated that they were not qualified or did not care to render a judgment in the matter. Apparently the older respondents have greater difficulty in accepting or making an evaluation of the Kinsey study. The relationship between increase in age of the respondent and the failure to accept the research may result from the fact that the older generation was reared in a more Puritan age and is now in a less active period sexually.

At the end of the questionnaire the following statement was given, "If you are willing to be identified as holding your opinions, please sign below, otherwise your identity will be held in strictest confidence." A space was provided for the respondent's signature. Of the 295 respondents 74 per cent indicated by adding their signature that they were willing to be identified publicly as holding their expressed viewpoint, and 26 per cent were not willing to be identified. When the responses were analyzed by sex of the respondent, three out of every four males were willing to be identified publicly as holding their expressed viewpoint, while two out of every three females were willing to be so identified.

DISCUSSION

Some of the comments and criticisms given by the respondents are not in accord with fact either as presented in the Kinsey report or as available elsewhere. The purpose of this discussion is to point out such inconsistencies and in addition call attention to comments which appear to be very questionable. It is not the intent of the writer, however, to enter into a discussion of the more debatable criticisms given by the respondents. Such criticisms can best be considered by the Kinsey staff.

The several criticisms regarding the publicity which accompanied the release of the Kinsey report indicate that some of the respondents were not aware of certain facts concerning its publication, as well as certain general practices of publishers. Obviously the author of a book selects his publisher. Kinsey released his first progress report to W. B. Saunders & Company of Philadelphia, which is one of the oldest publishers of medical books in the United States. It is doubtful whether a more conservative or

reputable publisher could have been found. The publisher and not the author determines the amount of money spent in promoting the sales of a book. The writer has never heard of a psychologist who has written a book and then requested that it not be advertised. On the other hand, it is common knowledge that many psychologists have been disappointed with the sales promotion of their books and that they have encouraged their publishers to increase publicity expenditures. According to Kinsey's statement, no agreement was made with the publisher concerning the amount of money to be spent in advertising the book. W. B. Saunders and Company has had complete control over the advertising. The publisher anticipated a first printing demand for the Kinsey report at 50,000 copies and scheduled production based on this estimate. The subsequent demand for the book was explained by some respondents as resulting from an intensive publicity program. The publishing world knows that advertising alone cannot create such a demand for a book.

Another criticism concerning publicity matters referred to the so-called "sensational" nature of the advertising which W. B. Saunders used when the book was released. The writer has failed to locate any advertisement which contained more than a statement of the title of the book, the names of the authors, the names of some of the sponsoring organizations, the publisher and a very concise statement as to the nature of the report. If such advertisements constitute sensational publicity, then practically all psychological and psychiatric publishing notices fall into this classification. Another comment stated that it became a best-seller because of its "sensational" title. One needs only to glance over the titles of the tremendous number of psychological and psychiatric books to discover many other titles which appear to be equally "sensational" in nature. For example, G. W. Henry's *Sexual Variants*, W. Stekel's *Bi-Sexual Love*, and F. Lundberg and M. F. Farnham's, *Modern Woman, the Lost Sex*. If sensational titles insured the number of sales of a book, certainly the ones such as quoted above as well as hundreds of others should have equaled or exceeded the sales of the Kinsey report.

Some of the comments referred to the limitations inherent in the population sampled in the Kinsey research. The data presented in the Kinsey report are based upon 5,300 white American males. In addition, the Kinsey staff had taken over 12,000 histories in all, and undoubtedly their familiarity with their unpublished data enhanced the treatment and evaluation of the specific data drawn from the 5,300 samples. The criticism that the study was restricted in its value because of limitations in the size of the sample is, without doubt, valid. Kinsey is in utmost agreement with those who desire to see the sample enlarged so that greater confidence and wider interpretations can be made from the data.

Kinsey states in the report, "It is now estimated that 100,000 histories will be necessary to carry out such a project" (3, p. 6). However, with such limitations recognized, there is undoubtedly considerable value in publishing the results obtained from 5,300 cases. If a population of this size does not warrant publication, then practically all the research concerning human behavior which has previously been done would have to be disregarded. For instance, Terman's volume on *Sex and Personality* was based upon only 94 males and 99 females.

A second group of criticisms concerning the size of the population includes those which suggest that the size of the population studied is entirely adequate and that in the future Kinsey should restrict his work to more intensive studies of a few individuals. Some of the respondents who offered this criticism were interested in having Kinsey explore certain theoretical constructs concerning psycho-sexual development which were not included in the experimental design of the Kinsey research. It is evident that each researcher must first determine the limits of his study, as it is obviously impossible to study all things at once. Scientific method involves the fragmentation of the universe and an intensive study of separate elements. The limits set on a study by any investigator may or may

not be most significant or wise, but seldom can contemporary researchers evaluate this aspect of research. Those who feel that Kinsey has omitted crucial areas in his investigations of human sexual behavior should find it a challenge to pursue their hunches. It is apparent that the investigation of human sexual behavior is of such a complex order that it cannot be completely or adequately investigated by any one researcher or any one research staff. The study of human sexuality is a scientific problem and is the province of all scientists who can and care to investigate it.

Another set of comments concern the statistical treatment of the Kinsey data. Some of these will undoubtedly contribute to a refinement of the study, while certain ones show the respondent's lack of orientation to the statistical procedures involved in the Kinsey study, or their naiveté in statistical matters. Several individuals have criticized the Kinsey report for using means instead of medians in the treatment of his data. This unjustified criticism has been popularized and accepted uncritically. An investigation of the statistical tables in the Kinsey report reveals that both the *median* and the *mean* are given practically every time it was logical to calculate both statistics. The mean was used exclusively in certain cases where the very nature of the data excluded the application of the median. For example, the median was not used when more than half of the population is in a zero class. Moreover, it was impossible to make a comparison of several groups by way of medians if the median for any single group is flat zero. Frequently in Kinsey's discussion of the data, he did employ the mean instead of the median. The use of the mean as the statistic for interpreting central tendency is a common practice in contemporary scientific reporting, and is probably the best understood of such measures.

The comment was made that Kinsey's data would have a deleterious effect upon youth. This assumption poses a very important research problem, and evidence bearing upon such a statement is certainly needed. In the meanwhile, psychologists should not accept such an assumption uncritically. Specific evidence regarding the opinion should be sought. The writer believes that the publication of fact can only have beneficial effects upon society, although the re-evaluation or re-organization of values and patterns of behavior which such facts demand may not be accomplished without some pain and struggle.

Some criticize Kinsey for not exploring certain concepts inherent in certain theories of psycho-sexuality. Kinsey clearly describes the purpose of his study as being a fact-finding survey of what people do sexually. He did not set out to investigate any specific theories of human sexuality. Scientifically it seems perfectly justified to determine the "what" before investigating the "how" in this particular problem. The data accumulated by the Kinsey research staff should do much to clarify many of the theoretical explanations of human sexual behavior which have been accumulating during the past 2,000 years. Those who feel Kinsey's approach has disregarded certain promising theoretical leads certainly have the privilege of following their own investigatory hunches.

Among the comments were a few which criticized Kinsey for not utilizing specialists in other fields than biology. Kinsey points out in his book that hundreds of specialists of all types have been consulted during the ten years which the study has been under way. Many nationally known scholars have actually spent days in the Kinsey laboratory at Indiana University. These consultants have studied the data intensively, contributed to the experimental design of the study, suggested modifications of procedures, investigated the statistical problems involved and added in numerous other ways to the refinement of the study. Actually thousands of hours have been spent by the Kinsey staff with specialists representing many areas of knowledge. Kinsey reports in his book that consultants were drawn from at least 33 contingent areas.

The criticism was made that Kinsey, being a zoologist, was therefore not qualified to conduct a study which involved the handling of psychological materials. If such a criticism were valid,

then psychologists would have to exclude the work of such scientists as I. P. Pavlov, Sigmund Freud, Havelock Ellis and numerous others, since they were not psychologists. The basic question involved here is whether or not the investigator is qualified to do the research. In a study of human sexual behavior, it is apparent that a large segment of the study would involve biological determinants. Kinsey's position as one of the foremost zoologists in the United States should certainly qualify him for this aspect of the investigation. Dr. Kinsey's specific qualifications in the field of psychology first consist of an undergraduate major in psychology. This reveals his long standing interest in behavioral problems. Secondly, his ten years of experience in the investigation of human sexual behavior should have contributed immensely to his ability for handling psychological data. There are few, if any, psychologists who during the last ten years have had more intensive and extensive experience in handling human behavioral materials than that which has been acquired by Dr. Kinsey. Some individuals criticized the study because psychologically trained consultants had not been added to the staff. Such criticisms are contrary to fact, as Dr. Vincent Nowlis has served on the Kinsey research staff for over four years. Also Mr. Wardell Pomeroy, who has served on the staff for over six years, holds an M.A. degree in psychology and had acquired several years of clinical experience before he joined the Kinsey staff. Dr. Kinsey is actively seeking specialists from several fields including psychology in order to increase the range of the professional talent on the staff. Only as additional research funds have become available, has such an expansion been possible. Dr. Paul Gebhard, who received his training as an anthropologist under Kluckhohn and Hooten, was added to the staff over two years ago. Other specialists will be added to the staff as the study progresses.

A few criticisms were to the effect that the Kinsey interpretations were not in accord with his published data. In all such comments not a single specific example was cited, and therefore no evaluation can be made or is demanded of such generalized criticisms.

Summary

In order to obtain an over-all evaluation of the Kinsey research project from a group of qualified psychologists, a mailed questionnaire was sent to every Fellow member of the Division of Clinical and Abnormal Psychology of the *American Psychological Association*. The question presented on the ballot was, "*Do you or do you not believe that the Kinsey research project is sufficiently worthwhile to be continued to completion?*" The response categories "Yes" and "No" were provided. This was followed by the statement, "Comment if you wish." Of the 330 questionnaires sent, 295 or 90 per cent of them were returned. Eight per cent of the males and 16 per cent of the females failed to return ballots.

1. Approximately 95 per cent of all psychologists surveyed in this study considered the Kinsey research project sufficiently worthwhile to be continued to completion. Only 2 per cent of all those surveyed indicated their over-all disapproval of the research.
2. There were 97 per cent of the male respondents and 87 per cent of the females who approved the research in the manner described.
3. When the responses were analyzed according to age of the respondent, it was found that 97.4 per cent of all those born since 1900 gave a "Yes" response to the survey question. Of those born before 1900, only 88 per cent answered "Yes." Of the remaining 12 per cent in this group, 4.3 per cent gave a "No" response, and 7.7 per cent declined to answer the question.
4. The respondents were asked if they were willing to be identified publicly as holding the evaluation and opinion which they expressed in their returned questionnaire. Approximately 76 per cent of the males indicated that they were willing to have their responses made public,

while only 66 per cent of the females agreed.

There were 156 of the 295 respondents (52 per cent) who gave an additional response under the heading, "Comment if you wish." Responses so obtained were classified, and representative examples of each classification were quoted. The writer discussed some of the points raised by their comments which appeared to be contrary to fact or based upon a lack of understanding of the Kinsey research project.

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ON THE TEACHING OF CLINICAL PSYCHOLOGY*

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In speaking of the teaching of clinical psychology, I shall not present any experimental findings for your edification. My own experimentation has never been concerned with problems of teaching, and I hold the strong personal opinion that most of the experimental work in the field, while far from sterile, concerns itself primarily with questions of methodology to the exclusion of more basic philosophical questions of content and educational goals which are in need of consideration before we can effectively apply the results of any experimental investigation of methods of classroom communication. Recently, I have seen the data of one experimental investigation of a teacher training program which seemed to have accomplished excellent results with one exception—the trainees failed signally in stimulating their pupils to independent thinking. While many of my authoritarian colleagues will not be alarmed by this, many others, including myself (and we are solidly supported by the findings of non-directive therapy), will find this a serious delinquency. Many problems of basic educational philosophy remain to be at least partially explored before we can too confidently trust ourselves to the use of any *method* of classroom communication, however sound its experimental validation. Among these are the relative value of a broad, historically oriented, eclectic point of view in classroom presentation versus a narrow one-system, intellectually provincial presentation; as well as the relative value of the culturally oriented, liberal arts approach in education as opposed to the narrower, pre-vocational orientation of many of our present day curricula.

The remarks to follow will concern themselves with a discussion of content in the teaching of clinical psychology, and will be limited to one type of content—namely, the dynamics of human personality. Clinical psychology as a profession is based squarely upon the facts of personality dynamics and has long taken pride in its leadership and in the breadth and depth of its contributions, both experimental and theoretical, in this field. Yet many of us in clinical psychology to-

* Address of the retiring president, Division on the Teaching of Psychology, American Psychological Association, Denver, Colorado, September 9, 1949.

day feel that our training programs need more of this approach, that our students need more training in the fundamental dynamics of normal personality the basic understanding of which underlies all the specific methodological practices, diagnostic and therapeutic, of our profession.

How can we supply this need? Any teacher would be false to the vested interests of the profession did he not mention the possibility of "adding more courses," but my own solution would be to add more personality dynamics to our present courses. In our formal classroom presentation of psychological findings, as well as in our personal thinking about them, we should add to our consideration of the factual and theoretical context in which we usually present psychology some consideration of the personal needs, strivings, and developmental history of the psychologists involved as well as the broader cultural trends reflected in these psychologists' needs and strivings. Our knowledge of the dynamics of human personality offers, just as Logic does, an interpretive framework against which to evaluate the materials of psychology, since such materials, experimental or theoretical, are the productions of very human, individual psychologists, and cannot help but be shaped, in greater or lesser degree, by the matrix of that personality of which they are a function. In essence, I am suggesting that our psychological thinking (or our psychological behavior) be subjected to the clinical insight of the diagnostic interview, that we practice our clinical profession with psychology as the patient. Parenthetically, this reaffirms my personal prejudice that diagnosis must precede therapy. The promulgation of a psychological theory, or even the choice of an experimental design, has its ultimate roots just as firmly anchored in the dynamics of a particular individual as does a bit of compulsive behavior or a pathological obsession. There may be a difference in the amount of socialization exhibited by these widely separated behaviors but they all spring from a basic matrix of human needs and desires.

The application of our clinical knowledge to the interpretation of human behavior is nothing new, it is our very stock in trade, but so far we have been remarkably persistent in applying it to the man in the street, the patient in the office, the physicist in the laboratory, and the philosopher in print. We have been remarkably successful, nevertheless, in avoiding its application to ourselves as psychologists in psychology. To misuse a phrase of Max Meyer's, we treat clinical psychology as "the psychology of the other one." As a case in point, I recently read an able treatment of the projective movement in which the author stated that projective testing must be understood as a movement of protest. But it developed that in this case at least a "movement of protest" was to be understood entirely in terms of the inadequacies of the opposition. There was no mention of such factors as the insecurity, frustration, aggression, and defensive overcompensation which usually accompany a protest movement. Let me hasten to add that the entrenched opposition must also be understood not only in terms of its logical and practical deficiencies but also in terms of the social dynamics of institutional perseveration.

Where such a "clinical" treatment does creep into our consideration of psychology (as in Cureton's recent discussion of "The Principal Compulsions of Factor-analysts"), it is usually developed humorously. I would suggest that it may be a more deadly business than we dare realize, and that we might well dispense with the defensive mechanism of humor. An outstanding exception to this general neglect of the motivational factor in psychology is Reik's recent "Listening with the Third Ear" which contains many incidental instances of Freud's understanding of his own motivations and a remarkable amount of candid self-revelation by Reik himself. My general thesis is stated in his words, "Psychology does not begin as self-observation. It ends there."

The distortion of reason by emotion has long been recognized, and has led the logicians to a distinction between cognitive and emotive communications. When in the course of their famous "Battle of Behaviorism" the late Professor McDougall refers to the stalwart Dr. Wat-

son as the antichrist and Dr. Watson answers tauntingly that Professor McDougall is returning to religion, it is obvious that more is involved than a mere rational consideration of the implications of their respective scientific productions. They are "mad" at each other. Personal, ego-involved "beliefs" are at stake and the resulting "argument" is not to be understood completely in terms of the logical consideration of certain scientific facts, but must be interpreted in terms of the personality structures of two positive individuals. For a complete understanding of this controversy we need a developmental history of both the organisms involved.

I have never found a colleague or student who failed to recognize this, yet nowhere in the literature can I find an adequate treatment of either behaviorism or hormic psychology from the standpoint of the personalities of the two men who so obviously influenced the development of these "schools." As psychologists we are committed to the basic principle that personality influences behavior, but in practice we grant our colleagues the professional courtesy of not applying this principle within the psychological family itself. We grant them a "presumption of depersonalization," a depersonalization which often is as unreal as the clinical depersonalizations we are called upon to handle in our patients. In overlooking the influence of personality on science, our attitude is strongly reminiscent of the Victorian attitude of American psychiatry during the first quarter of this century to the "unpleasant" realities of psychosexual development. Yet the behavioral phenomena exist, and cannot be removed by the comfortable mechanisms of repression.

The phenomena of obviously emotive communications, however, constitute only a small part of the logical distortions that creep into our scientific thinking as a function of our personal dynamics. More interesting are the subtle distortions of cognitive thinking that are produced by motivational forces intruding from outside the logical framework of scientific facts available for consideration in the solution of any problem. I am assuming that science is relative, that it deals with the evaluation of probabilities rather than the discovery of absolutes, and that the scientist must always build his theoretical structures upon the shifting sands of ever increasing experimental findings. At any moment we may *act* after calculating our probabilities, but we must act with the clear recognition that our act is a gamble. As Reichenbach says, "We posit the event to which the highest probability belongs as that event which will happen. We do not thereby say that we are convinced of its happening, that the proposition about its happening is true; we only decide to *deal with it* as a true proposition."

While this relativism is all that we have as scientists, it seems not enough for us as human beings. Committed to the relative, we seek the absolute. Committed to partial knowledge, we strive for omniscience. Committed to an experimental basis for our science, we struggle to close by rational interpretation the gaps left open by our meager experimental findings. It is one aspect of that universal human phenomenon that my friend, T. H. Howells, has so aptly labeled the "hunger for wholiness." Our relatives, the philosophers, are outgrowing this lust after cosmology, but psychology is still smitten with the absolute. It is just here, where we transcend our experimental findings, that motivation is particularly apt to intrude to color and distort our thinking. Our experimental findings are the reality factors, the external world of objectivity, under whose control our intellectual development must proceed; when we go beyond them the pleasure principle rears its ugly head, and the path is open for a regression into the tempting world of autistic phantasy.

One of the most apt characterizations of this tendency that I have ever heard was given by Professor E. G. Boring while lecturing on McDougall in one of his classes. Boring was discussing his early association with McDougall at Harvard and their frequent attempts to resolve their differences of opinion. Boring mentioned the area of uncertainty or area of unpredictability that always remains even after the nicest of experi-

ments. Boring said that he suddenly realized that the root of his differences with McDougall lay in this area of uncertainty: "I called it probable error. McDougall called it God." In essence it is the same error later committed by Eddington and Jeans when they derived deity from Heisenberg's principle of uncertainty. It is the old familiar fallacy that what cannot be proven untrue is therefore true, because we wish it so.

When we as theoretical thinkers transcend the experimental findings of our laboratories, there is much in our thinking processes that bears analogy with the delusional thinking of the psychotic. As clinicians we are all familiar with the developing psychotic process that strives to assert its claims while the patient struggles to still maintain himself within the framework of reality. The reality principle still governs the developing autistic trends that seek constantly to find some weak link in the chain that binds the personality to the stern demands of reality. Then finally the weak link is found, perhaps in spiritualism, perhaps in religion, perhaps in some garbled press statement of scientific findings; and the harried personality finally breaks loose from reality and departs upon its pleasant regressive journey.

As psychologists we constantly face this never-ending struggle between the sternly limiting experimental findings of our science and the fierce drive of our personal needs and motivations. Too often the psychologist, like the schizophrenic, finds in some "area of uncertainty" or in the misinterpretation of some experimental finding the final excuse for turning away from the grim confines of scientific reality to the pleasant fields of autistic affirmation. It would be unrealistic of us as scientists to hope that such a state of affairs can ever be completely avoided. The human organism must be accepted as it is. But we can be aware of such motivational distortions, and can use our understanding in evaluating them. It is here, in applying our knowledge of motivational forces to the understanding of psychology itself, that clinical psychology can make a great contribution in the classroom. We must examine our own motives and their influence upon our psychological behavior. We must ask our classical colleagues "Why a rat man?" and then turn to ourselves and ask "Why a clinician?" The answer will always involve our developmental history as emotional organisms. Someday a new Freud may write a new "Future of an Illusion," dealing with psychological theory as a defensive mechanism against insecurity and the demands of the cold, hostile world of nature. Today we can only point to the problem.

Lest it appear that I am denying the place of theory in science, let me hasten to distinguish between what I may call the "open" system which follows closely the scientific rules of the game in achieving a tentative organization of experimental findings, fertilizes the production of new findings, and eagerly adapts itself to such new findings; and the "closed" system, or System with a capital "S," which attempts a final and ultimate organization, directs its experimental efforts toward only such findings as will reaffirm the system, and bitterly resists any developmental change. Let me also point out that in a world which contains a definable black and a definable white there exist as well an infinite number of greys.

What I am saying is that science is a means of achieving *relative* knowledge, but that human motivation continually misuses it for the production of *absolutes*. If the twentieth century will be marked in history by any single contribution, I feel that it will not be atomic fission, but rather Einstein's development of the principle of relativity and our generalization of this into a basic code of scientific thinking. Twentieth century man will emerge as *relative* man and his characteristic neurosis, anxiety, will be seen to depend in large part upon the necessity of abandoning his reliance upon a system of fixed and unchanging beliefs in order that he may live adaptively in a rapidly changing world. Where nineteenth century man awoke and faced each day secure in the possession of a series of fixed principles which would guide his actions throughout that day, twentieth century man awakes to no such comfort. Each

day he must face anew the problems of living and seek fresh judgments and fresh solutions which incorporate the onrushing developments of science. His course of action for the day cannot be selected until he has digested not only his breakfast but the morning press as well (and I add the pious hope that it may be the *New York Times*).

The misuse of "Systematic" absolutes reveals its motivational origins when we compare it with related neurotic and psychotic mechanisms also used to handle anxiety. Here is the compulsive's old friend verbal exorcism, where the anxiety producing properties of the probable error inherent in some learning experiment are rendered harmless and neutral by the mystical muttering of the magical words "secondary reinforcement." Here is forgetting and the repression of the unpleasant which leads our comparative colleagues to imitate the tubular vision of the hysteric and concentrate their attention upon anal derivatives in the hoarding behavior of the white rat to the complete exclusion of the problems surrounding the human collection of scientific data and its sublimational manipulation by due process of current statistics. There is also the phenomenon of contamination and interpenetration (familiar in our Rorschach practice) when we find nondirective therapy becoming confused with democracy and the defense of the Constitution of the United States. As for the use of neologisms—no wanderer in the mazes of current professional literature needs to have this pointed out to him. Last, but by no means least, is the appearance of inappropriate emotion, whose turbulence ill becomes the public-library dignity of our scientific halls.

It has always seemed to me that the dogmatism and intolerance of American behaviorism from Watson to Skinner reveals an emotional potential above and beyond that to be expected in the pursuits of pure science. Such an emotional charge suggests a *need* to be right, a need not springing from any narrow personal involvement, but from a deep desire to construct a basic system for the conduct of life and the control of society. To answer such a need the system *must* be simple, workable, and at the same time infallible. Otherwise chaos may ensue, society may not be saved, and individual anxiety cannot be handled. In surveying Behaviorism one feels that the basic motivation of the experimental work stems from the wish for a planned society, rather than the planned society following logically from the experimental work. Watson's later work and subsequent writing was definitely directed toward the practical control of human behavior, and Skinner followed his theoretical treatise with a literary formulation of a new behavioristic Utopia. In reading Skinner's "The Behavior of Organisms" and his "Walden II," one cannot help suspecting that the priority exhibited in the order of their publication is perhaps reversed in the author's unconscious.

The same spirit runs through much of American social psychology. In reviewing the work of my "Spissy" colleagues in the Society for the Psychological Study of Social Issues, I am reminded of Russell's comment on philosophers: "Knowing, as they supposed, what beliefs would make men virtuous, they have invented arguments, often very sophistical, to prove that these beliefs are true." The mission of American social psychology too often has been not a scientific one, but rather a religious one involving the experimental reinforcement of an *a priori* social ethic. In the deliberate choice of an experimental problem to be investigated, in the unconscious choice of experimental design, and in the sweeping generalizations often drawn from meager objective findings, this crusading social spirit is continually revealed. It has been marked not so much by good science as by science in a good cause. We are engaged not in the further discovery of the unknown, but in the justification of an *a priori* "given." One cannot help feeling that social psychology today is in direct line of descent emotionally from New England transcendentalism and has inherited its ethical tradition. My own membership in the Society and my participation in the tradition attest to my approval of the ethic involved, but I cannot help but feel that I am behaving more as an emotional human being than as a

cold, impartial scientist. Both ethics and science have their place, but a confusion of their respective problems can easily lead to disaster in a world balanced as delicately as is ours today. The whole of social psychology can be viewed as a vast projective test whose results need careful clinical interpretation before they can safely be translated into social action.

I would be false to my own thesis did I not point out in closing that the above remarks also must be interpreted in *their* psychological setting. They are not an impersonal statement of experimental findings. They are the very personal interpretations of a human organism which has had its own particular developmental history, and they must be heard as such. Their evaluation I leave to their audience. It is a common saying that the physician cannot heal himself.

These are fundamentally problems of human dynamics, of the motivational matrix of human needs and strivings out of which our psychological theory and experimentation have come. It is high time that psychology turns its interpretive insights on itself. This is a task for which clinical psychology is prepared, and a responsibility which the teacher of clinical psychology should be ready to assume. If we are successful, not only would psychology as a whole be benefited, but our special discipline of clinical psychology would be encouraged toward those broader horizons that we occasionally caught sight of during the recent weeks of the Boulder Conference.

RESEARCH TRENDS IN CLINICAL PSYCHOLOGY*

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Each January, the *American Journal of Psychiatry* carries a review of psychiatric progress for the preceding year. Writing for this review, Stanley G. Estes[3] was able to delineate progress in the field of psychometrics in 1947 with only a 20-item bibliography. For 1948, he accomplished this task by reference to only 18 papers[4]. For neither year were all of his references to *research* publications. Of certain areas of research, *e.g.*, structured personality inventories, he was extremely negligent. Aside from the question of representativeness, if the quantity aspect of Estes' reviews were to bear any correlation to the true level of research activity, clinicians would have sufficient cause for concern.

In an effort to achieve a more representative evaluation of research trends in the field of clinical psychology the writer has chosen to go directly to the journal literature, on the assumption that it is the most direct expression of *actual* research activity as contrasted with "vest pocket projects." This survey has been guided by an operational definition of clinical psychology as consisting of the activities of persons who work in face-to-face relationships with clients having problems primarily in the area of personality and adjustment. Such psychologists are concerned with the research development, administration and interpretation of psychometric instruments designed to describe and measure maladjustment and its psychological correlates. They are concerned also with the development, application, and, in particular, the evaluation of techniques of therapy.

The survey of the literature has been limited to the four journals most likely to carry research studies pertinent to clinical psychology as earlier defined: *Journal of Clinical Psychology; Journal of Consulting Psychology; Journal of Abnormal and Social Psychology;* and *Journal of Applied Psychology.* The writer attempted a cursory tabulation under appropriate rubrics of the research contents

* From a paper given at the First Annual Conference of the Minnesota Psychological Association, Minneapolis, April 1, 1949.

TABLE 1. *Types and frequency of research in clinical psychology appearing in four journals,* 1946-48*

	1946	1947	1948	1949†	1946-1948 Total	Per cent of total	Rank order of frequency
Normative study (personality)	14	15	7	3	36	23.4	1
Scoring technique (struc. & proj.) ..	5	7	3	1	15	9.7	2
Validity study (projective)	3	5	3	..	11	7.1	3
Normative study (intelligence)	5	3	2	..	10	6.5	4
New projective tests	1	5	3	1	9	5.8	5.5
Normative study (deterioration)	2	6	1	..	9	5.8	5.5
Wechsler pattern analysis	2	2	4	2	8	5.2	7
Test-taking attitude (personality) ...	1	1	5	..	7	4.5	8
Abbreviated intelligence test	1	1	4	1	6	3.9	9.25
Intertest relationships	1	1	4	1	6	3.9	9.25
Analysis of test structure	1	2	3	..	6	3.9	9.25
New scales (personality)	3	1	2	..	6	3.9	9.25
Psychol. of physically handicapped ...	1	1	3	..	5	3.2	11
Analysis of recorded interviews	..	1	3	1	4	2.6	12.5
Test-retest in normal, neurotics	..	2	2	1	4	2.6	12.5
Personality correl. of physiol. changes	..	..	3	..	3	1.9	14.3
New test (deterioration)	2	..	1	..	3	1.9	14.3
Test-retest of lobotomy	..	2	1	..	3	1.9	14.3
Test-taking behavior (Rorschach) ...	..	..	2	..	2	1.3	16
Non-direc. Rx, specific group	..	..	1	..	1	.6	17

* J. clin. Psychol.(8) J. consult. Psychol.(9) J. abn. soc. Psychol.(6) J. appl. Psychol.(7)
† First three months.

of these publications for the three post-war years, 1946 through 1948. The classification was essentially empirical rather than *a priori,* developing as the effort at tabulation proceeded. Twenty relatively distinct categories evolved although they are not highly refined, and the placement of specific research studies was in some instances essentially arbitrary. Only those articles were classified which were clearly of a research nature, *i.e.,* involving the systematic investigation of a specifically described group of subjects and the derivation of normative or comparative data from psychometrics, case histories, or therapeutic interviews, or analysis of the mechanics of administration, scoring, and interpretation of a given instrument.

An analysis of the 154 tabulated studies (Table 1) yields several points worthy of comment. In the first place, a crude estimate was made of the total number of separate articles, exclusive of book reviews and short notes, carried by the four journals during the three-year interval. This estimate was based on an extrapolation from the exact data for 1946 and is probably an underestimate of the true total. The tabulated research studies constitute approximately 20 per cent of this estimate of the total number of published articles. The true proportion is probably less than 20 per cent inasmuch as the estimate of the total is probably somewhat niggardly rather than generous. It appears that over the three-year interval *less than one-fifth* of the contents of these four journals was devoted to actual researches in clinical psychology.

To some extent this very small proportion of research publications might be explained as due to the post-war period of civilian relocation of clinicians during which time research activities were temporarily in abeyance. Were this true, it would be expected that research activities would be primarily conceived during 1946, labored over in 1947, and published in 1948. This expected pattern of research activities would be verified by an essentially straight-line function of marked positive slope when number of studies is plotted against the three-year interval. Actually, the tabulation shows a total of 42 published researches in 1946, 55 in 1947, and 57 in 1948. The proportion of the tabulated researches published in 1948 is less than 10 per cent greater than the proportion contributed in 1946.

These data suggest that a plateau, let us hope a temporary one, has been reached at a point which indicates only slightly greater productivity than that of the first post-war year. It must be recognized that an hiatus in research publication resulting from the war, together with publication lag, may fully account for the level of productivity suggested by the contents of the four journals, and that 1949 or 1950 will show a great spurt in research. It is notable, however, that during the relatively arid interval which has been surveyed there is evidence that clinical psychologists have found time and motivation to engage in various symposia, to write numerous articles on problems of administration, training, ethics, interprofessional relations, and certification, and to contribute testimonials on new techniques. Does this mean that the incentive of wide government subsidy has served to make our field of psychology *more* conscious of its professional status than of its basic research and service responsibilities?

The distribution of the 154 studies by general type of research gives reason for concern in terms both of the types of research having high frequency and the types having extremely poor representation. The category having the greatest number of studies is, reasonably enough, that labeled normative studies of personality, in which were tabulated all researches involving the investigation of a particular group or groups of subjects by means of a specific instrument and the publication of statistical norms. This category accounted for slightly over one-fifth of all the studies. Strangely, however, the second most frequent category, one accounting for nearly 10 per cent of the tabulated studies, is that labeled "scoring techniques." If this category represented exclusively studies devoted to the objectification and reliability of projective instruments, in which the scoring of responses is a primary and difficult problem, there would be little cause for concern. The fact is, however, that a considerable portion of the articles represented by this category are expositions of supposed shortcuts in the scoring of such structured instruments as the Minnesota Multiphasic and the Bernreuter inventories. The Multiphasic has particularly heavy representation in this category. Perhaps Corsini's[2] recent time-and-motion study of the scoring of this instrument and his demonstration of the comparative efficiency of the originally published scoring procedures will discourage clinicians from further efforts of this oblique sort and channel their endeavors into the more crucial problems of cross-validation.

The third most frequent type of research was concerned with the validation of projective techniques. Aside from the fact that the validity of an observational tool always is a respectable concern for any psychologist, it would appear to be a particularly healthy sign that the recent literature shows several efforts to experimentally test the basic postulates concerning the behavioral correlates of responses to such controversial instruments as the Rorschach and TAT. Williams'[10] attempt to establish the dynamic relationship between certain of the Rorschach variables and adjustment to an operationally defined stressful situation has been deservedly commended as furnishing an excellent paradigm of the type of validity study which projective devices sorely need. Only after an operationally adequate demonstration of the validity or non-validity of the theoretical correlates of projective responses can there be hope for agreements in interpretation which will permit investigation of the clinical validities of projective instruments on a more rational basis than has previously been possible. It is to be hoped that enthusiasm for such studies will not encourage undue generalizations of their results to non-college samples and negligence of the need for cross-validation even at this relatively basic level.

Enthusiasm for this apparent trend toward experimental validation of the older projective techniques is dampened by recognition of the rapidity with which new projective devices are being proposed. The fifth category of research studies in order of frequency is that of "new projective tests," the data indicating an average of three new devices per year over the three-year interval. In view of

the fact that the nearly thirty-year-old Rorschach test is only now undergoing rigid experimental validation, one is not unduly skeptical to wonder if the practical validation of existing projective techniques will ever catch up with the facile proposals of new ones. Perhaps workers of the future will look back on clinical psychologists of this period chiefly to marvel at our propensity for the construction of incomplete sentences!

To round out a picture of the areas in which research has been concentrated, mention should be made of the remaining categories which rank among the first ten in terms of frequency and the per cent of the total number of researches which they include. These categories are, in order of frequency: normative studies of intelligence (6.5 per cent); normative studies of intellectual deterioration or brain damage (5.8 per cent); Wechsler pattern analysis in personality diagnosis (5.2 per cent); test-taking attitude as reflected in personality measures (4.5 per cent); abbreviated intelligence tests (3.9 per cent); and inter-test relationships (3.9 per cent).

Clinicians who work in a medical setting will be particularly impressed by the small amount of attention being given to the development of techniques for the detection of subtle psychological deficits of organic origin. This is admittedly an extremely difficult research problem yet one of great clinical importance. The alert neurologist eagerly seeks the assistance of the psychologist in the evaluation of those frequent cases in which there is a suspicion of encephalopathy although the neurological procedures yield negative or at best ambiguous findings. None of the existent deterioration indices is entirely satisfactory. The clinician who can perfect a practical, reliable and reasonably fool-proof instrument or battery to fill this need will find it greeted by a reception equal in warmth to that accorded the Binet-Simon scale. Yet, it appears that slightly more than one-twentieth of our research energies are being directed to the satisfying of this pressing clinical need.

Particularly notable by their relative absence are researches designed to measure the effects of specified forms of psychotherapy, to develop or investigate criteria of personal adjustment or changes in such adjustment, to analyze the comparative test-retest behavior of personality tests in normals, untreated patient, and treated patient groups. The supposed stimulus to research on the structure and movement of therapeutic interviews provided by the work of Rogers and his students has not yet been seriously reflected in published research by members of the opposing camp. The writer recently attended a national symposium on counseling and psychotherapy which was sponsored by the University of Illinois. Not until the evening of the first full day of the symposium, with the presentation of the sixth from a total of fourteen papers, did he hear the detailed report of actual research concerned with the development, reliability study and validation of a criterion for measuring the effects of a therapeutic program. Significantly, this study[5] was not done in a psychological clinic but rather in a social agency and involved not clinical psychologists but social workers.

The impetus to the field of clinical psychology which has arisen from the psychiatric burden of the war years has led most directly to federally and privately subsidized provision for the enlargement and improvement of training facilities. The immediate goal has been to increase the number of highly trained personnel. The ultimate goal, through the sufficiency of such personnel, is to increase the quantity and quality of research in the areas of mental hygiene and psychiatric therapy. In summarizing the fifty-year history of clinical psychology, from 1896 to 1946, Brotemarkle[1] made the following statement:

> In 1946 it is most encouraging to see at hand the developing possibility of more extensive clinical research. Our training programs now include opportunities for practicum training in the form of internships. Institutions, schools, business and other agencies offering internship opportunities, are presenting evidence not only of the opportunity for research, but their insistence that well-planned comprehensive research must be undertaken.

The optimism of this view does not appear to have been justified by the extent and nature of research which has appeared through 1948. Possibly it is still

too early for the apparent, current exhilaration of clinical psychology to take permanent form in research contributions. However, so far as the three immediate post-war years are concerned, and so far as the type of survey which has been attempted is adequately representative of the level and direction of overall research activities, clinical psychology would appear, to an uncomfortable degree, to be a *talking* rather than a *counting* science.

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REQUISITES FOR RESEARCH IN PSYCHOTHERAPY

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With the recent publication of the series of papers, *A coordinated research in psychotherapy,* by Rogers, Raskin, and their associates at the Counseling Center of the University of Chicago[4], psychotherapeutic research may be said, if not exactly to have come of age, at least to have got well beyond the teething stage and to have taken its first healthy strides. Certainly, nothing as mature as this series of research papers has hitherto appeared from any other group of therapists; and the painstaking attempts of the Chicago non-directive therapists to produce a coordinated research project in the best traditions of systematically planned, objectified psychological experimentation are noteworthy ones that, it is to be hoped, will have many repetitions by other therapists of various schools.

In view of the excellent start towards scientific research standards made by the Chicago group, it might seem presumptuous to cavil with the methodology or criteria employed in their published studies. Psychological progress, however, has largely consisted of a long series of cavilings with even the most noteworthy and advanced research papers and theoretical concepts; and Rogers and his associates are among the first to acknowledge the existing shortcomings of their methods and the wide latitude which they still have for improving these methods. The remainder of this paper will therefore be devoted, not to disputing or attempting to destroy the research methodology so effectively used in *A coordinated research in psychotherapy,* but to an attempt to suggest some additional research criteria over and above those outlined in the published papers. If, in the course of the ensuing discussion, the writer appears to be overly-critical of the research techniques of the Chicago group, this will be because these are the best available therapeutic research materials, and lend themselves far better for illustrative purposes than less explicitly defined and refined research methodologies. If the ensuing discussion also seems to be overly-critical of non-directive therapeutic researches, rather than those of other schools of therapy, that too is because the non-directive researchers have been most objective and progressive in their research presentations; moreover, the writer has recently criticized at length the research (or lack

of research) methodologies of the psychoanalytic therapists[1, 2].

To establish psychotherapeutic research on a scientific basis, Seeman suggests five methodological steps:

1. The electrical *recording* of cases.
2. The definition of *concepts* which provide an understanding of these cases.
3. The development of objective *measures* of these concepts.
4. The *application* of these measures to the same case material.
5. The interrelating of the results of this application in order to (a) establish the relationships existing between the concepts and (b) obtain a well-rounded picture of individual cases[4, p. 215—Italics Seeman's].

Accepting all these steps of Seeman's as appropriate and necessary ones for the accomplishing of scientific research in psychotherapy, it is suggested that certain other steps and procedures, which do not seem to have been adequately considered or outlined by the Chicago group, are also desirable and necessary for the performance of objective psychotherapeutic research. Some of these additional methodological points will now be outlined.

1. Psychological researchers in therapeutic areas must be especially cautious about naively accepting patients' statements at their face value. Introspective evidence has been suspect ever since the earliest days of psychological experimentation; but, for all its failings, it is utterly essential for investigations into therapy. Yet, the peculiar relevance of the patient's introspections to the therapeutic (and, in the case of therapeutic research, the *experimental*) conditions cannot for a moment be ignored: else scientific disaster, as far as the conclusions of any therapeutic research are concerned, is almost inevitably courted. That is to say: while the ordinary psychological subject is not too ego-involved in the experiments in which he agrees to partake, and therefore has relatively little incentive for (consciously or unconsciously) exaggerating or falsifying his reported introspections, the therapeutic subject is most intimately involved in what Sullivan and his associates[5] have called an interpersonal relationship with the therapist; and every single one of his statements is consequently to be strongly suspect of parataxic distortion, transference effects, rapport influences, or other incentives for distortion.

For example, in the Chicago studies, Sheerer finds that "there is a definite and substantial correlation between attitudes of acceptance of and respect for self and attitudes of acceptance of and respect for others"[4, p. 175]. She and her associates fail to consider the possibility that this observed relationship, as well as several similar ones found in the studies, may be more a function of consistent patient attitudes towards their counselors than towards themselves.

Again, Raskin notes that significant increases in self-attitudes, self-acceptance, insight, and behavior, along with decreases in defensiveness represent "a reorientation of the client's attitudes and behavior significant enough to be termed successful psychotherapy"[4, p. 215]. Actually, however, these *reported* attitudes and behavior of the patients may largely represent their efforts to obtain rapport with or love from their therapists; and, conceivably, they could represent overdependence, and perhaps *poorer* personality integration of the patients.

The main point to be made is that if investigators like Kinsey, who are frankly interested in their respondents for objective research purposes and who necessarily obtain only a mildly intimate relationship with most of their respondents, admittedly have difficulties obtaining perfectly truthful answers to ego-involving questions, psychotherapists who attain infinitely more complex and intimate relationships with their patients must under no circumstances accept these patients' statements at face value, but must always seriously consider the hypothesis that the significant relationships observed among their patients' *verbalizations* may in part be artifacts of the therapeutic relationship rather than objective evidences of

these patients' innermost attitudes and feelings.

2. In defining concepts for the understanding of therapeutic cases, care must be taken to make these concepts maximally incisive and meaningful. The line of least resistance, which so often results in the production of highly objective, and highly *un*important, psychological researches must be consistently fought and conquered. Thus, although some of the concepts, such as insight and defensiveness, investigated by the Chicago group are exceptionally meaningful and realistic in terms of actual problems of therapy, some of the other concepts, such as self attitudes, are rather vague and meaningless. To know, for example, that patients tend to make significantly fewer statements of their problems and offer more discussions of their plans in the closing than the opening sessions of therapy is all very interesting (though hardly surprising). More to the point, it would seem, would be to discover whether they make more or less use of such dynamisms as projection, rationalization, identification, and compensation as therapy proceeds. It is to be hoped that because the obtaining of information on the deeper dynamisms offers considerably more difficulty than obtaining information on simpler response categories, the former type of research is not to be neglected in favor of the latter.

3. For the proper understanding of research reports, as well as for the encouragement of experimental repetitions by subsequent researchers, fairly complete diagnostic or descriptive information on therapeutic research cases should be published along with the factual data. Unfortunately, the Chicago group has been surprisingly lax in this respect; and the result is a series of six research papers, presenting considerable data for analysis and discussion, and nowhere including even a cursory description of the ten patients whose protocols are repeatedly used in the investigations. The reader is told the age, sex, and number of interviews with each patient but virtually nothing else. Indications as to whether the patients were seriously disturbed or mildly upset, whether they were vocational, scholastic, or emotional referrals; whether they had any previous types of therapeutic sessions; whether they were satisfied or dissatisfied with their counselors; these and numerous other important questions concerning these patients are entirely unreported in *A coordinated research in psychotherapy*. Assuming that it is not the practice of the non-directive counselors involved in these researches to make specific diagnoses, it would still seem that the reader of their reports is entitled to clear-cut descriptions of (a) the patients' symptoms upon and reasons for entering therapy; (b) the main feelings and attitudes of the patients towards their therapists and the counseling process; and (c) the condition and attitudes of the patients at the close of therapy. If these facts are lacking, it will be almost impossible for the reader to form his own evaluations of the objective data presented, for the investigator to make suitable comparisons with other relevant therapeutic research studies, and for the would-be repeater of the investigation to equate his experimental subjects with or compare them to those employed by the original investigator. Even assuming, therefore, that psychiatric and psychological diagnosis is considered to be largely or entirely irrelevant for therapeutic purposes by a given school of therapists, it would appear that it is indispensable to this school's research investigations and reportings of therapeutic processes.

4. It is vitally important that research in psychotherapy be specifically planned *as research* from the start, and that it be carried out in a truly cooperative manner. Although the Chicago group's researches were very well coordinated, they admittedly "just growed," and were not originally planned in either a coordinated or a cooperative manner[4, p. 149]. More to the point, perhaps, they consisted

entirely of investigations of therapeutic *protocols* rather than therapy *patients*. Nor were they in any sense *experiments* in therapy; but, rather, *ex post facto analyses* of therapeutic sessions which had already run their normal course.

Psychological experimentation has suffered too long from what might be called Ph.D.—itis: namely, the selection of research problems which are relatively easy to plan, to get subjects for, to use standardized instruments with, and to amass neat-fitting quantified data about[3]. It is to be hoped that research in psychotherapy will avoid following this too well-beaten path and will concentrate more on the truly difficult, and most pressing, problems of therapy: such as, which psychotherapeutic techniques are the most effective with what kinds of patients, and why? Granted that the answering of such questions requires long-term research planning, execution, and follow-up by fairly large groups of cooperatively working experimenters (no one of whom may be able to get individual Ph.D. or publication credit), there seems to be no practical alternative for arriving at unequivocal answers to the most important therapeutic problems. While virtually all psychological research is, in the long run, instructive and useful, it is to be wondered whether contemporary researchers in therapy can afford the luxury of tackling relatively unimportant side issues when the solutions to the main ones are still so pressing and obscure.

5. The problem of sampling is just as important to research in therapy as to other psychological experimentation, and should be adequately recognized and tackled. Thus, the protocols analyzed in *A coordinated research in psychotherapy* were taken from the recorded cases of only ten subjects, three of whom had only three therapeutic sessions each and none of whom had more than nine sessions; these subjects all voluntarily accepted non-directive counseling, and nine out of ten were young graduate or undergraduate students. Apparently, therefore, the subjects of this coordinated research project consisted of a highly specialized very small non-representative group of patients; and the generalizations which Raskin[4, p. 215] makes about successful psychotherapy on the basis of studies of these patients are to be accepted with extreme caution. It is to be hoped that future therapeutic researches will include broader and more representative samplings of patients, or will confine their generalizations to the types of patients sampled.

6. While attempts at attaining quantification and reliability of psychotherapeutic research data are certainly to be encouraged to the extent that they are practical and meaningful, concomitant qualitative and sequential analyses of sessions of psychotherapy should also be considered. Just as projective techniques like the Rorschach test cannot be adequately interpreted only on the basis of the scoring determinants but also require content analysis, so do therapeutic sessions, to be fully evaluated and understood, require (a) contextual consideration of each session as a whole; (b) consideration of each session in relation to all the sessions of a given patient; and (c) consideration of each and all the sessions in relation to the general background of the patient's life and problems, all diagnostic and other information which is available on the patient, and the personality and problems of the therapist. While the contextual analysis of the patient's statements and behavior, and their interrelationship to the gestalts of his entire therapeutic and life processes, are admittedly difficult problems to undertake, it is hard to see how they can be eventually avoided if the major enigmas of psychotherapy are to be truly got at and solved.

Summary

It may be noted, in summary, that the recent coordinated research project in psychotherapy by the Chicago group of non-directive therapists is a most heartening prelude to what may be expected to be eventual outpouring of objective psy-

chotherapeutic researches. If these researches are to be maximally productive and meaningful, however, it is to be hoped that they will go beyond the already published studies in their outlook and their methodologies. Specifically, it is to be hoped that researchers in psychotherapy from many different schools will give due consideration to projects that seek beyond the mere verbalizations of the patients; that make use of maximally incisive and meaningful psychotherapeutic concepts; that include adequate diagnostic or descriptive information on the cases studied; that are planned from the start as controlled experiments in psychotherapy, and are executed in a truly cooperative manner; that take due account of sampling and generalizing to appropriate universes; and that, while paying adequate attention to questions of reliability and objective quantification, also carry out necessary contextual, configurational, and qualitative analyses of the data obtained from the recording of psychotherapeutic sessions.

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THE CONTENT OF MENTAL HYGIENE LITERATURE. II. THE NATURE OF ITS DIRECTIVES

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INTRODUCTION

A previous report[26] evaluated the contemporary status of bibliotherapy, established the *number of directives* per page in four categories of mental hygiene literature, and discussed the significance of the directive content. The purpose of this supplement is to study the *nature* of the directives in the same sampling of popular and academic writing.

METHOD

Analysis of the directives located in the sources listed in the biography of this paper, placed them in twenty categories.* A key word, a brief description when required, and an illustration of each category follows:

1. *Anxiety.* Worries, fears, phobias. "The worrier must substitute other behavior for worry"[2, p. 41].
2. *Efficiency.* Working quickly and effectively. "Clear your desk of all papers except those relating to the immediate problem at hand"[3, p. 213].
3. *Responsibility.* Assumption of role in determining life's course. "It is very important to avoid unwholesome attitudes of mind such as, 'What's the use of trying?' to feel sorry for oneself, shirk personal responsibility . . . seeking pleasure only . . ."[17, p. 26].
4. *Life goals.* What one should strive for. "Beware of having somebody to live for and making too much of that person; select an enterprise to live for much larger than yourself and more likely to outlive you"[9, p. 160].
5. *Methods.* Ways to solve problems. "Analyze your bad habit"[11, Vol. 22, No. 1, p. 3].
6. *Consultation.* The need for individual professional aid. "Should

* Detailed data concerning directives obtainable from author.

this not make you glad after all, consult a psychiatrist"(1, p. 90).

7. *Occupation.* "If you want a new sort of occupation, go get it"(9, p. 100).
8. *Persuasion.* Control over others. "'Don't' always calls attention to the forbidden act"(15, p. 398).
9. *Philosophy.* Religion, ethics, morals. "The sovereign cure for worry is religious faith"(6, p. 243).
10. *Pleasure.* Enjoyment of life. "Become aware of the present and try with all your energy to extract pleasure from it"(11, Vol. 23, No. 1, p. 33).
11. *Psychosomatics.* "Make sure you aren't punishing yourself for guilt by 'taking it out' on your stomach" (11, Vol. 22, No. 4, p. 35).
12. *Recreation.* "Enjoy enough rest and relaxation"(19, p. 539).
13. *Relaxation.* Release of muscular tension. "This (progressive relaxation) can be done by having the subject contract the muscles first, and then seeing whether he can experience the muscle sensation due to this contraction"(16, p. 356).
14. *Self-confidence.* "Assume an attitude of self-confidence"(20, p. 469).
15. *Self-diagnosis.* Evaluation of own mental health. "Jot down your skills and weaknesses, in two columns, then tackle the weaknesses"(11, Vol. 22, No. 4, p. 101).
16. *Self-tolerance.* Acceptance of "id" trends. "Acknowledge and face your anti-social impulses"(7, p. 23).
17. *Self-understanding.* Insight into dynamics of personality. "Don't forget that every symptom . . . is a symbol that stands for . . . some repression, complex, or conflict in the unconscious"(1, p. 90).
18. *Sex and marriage.* "If young women wish to retain their allure for their husbands they should have regard for sexual modesty"(13, p. 299).
19. *Sleep.* "Don't let insomnia put you in a panic; don't roll from one side of the bed to the other; don't try to stop thinking"(1, p. 115).
20. *Social.* Adjustment to friends, associates. "To make people like you, be a good listener; encourage others to talk about themselves"(4, p. 93).

A special count was made to determine the number of directives which were of

TABLE 1. *Per cent of directives devoted to various topics in four samplings of mental hygiene literature* †*

1 *Topics*	2 *Popular books*	3 *Popular journals*	4 *Academic books*	5 *Abnormal psychology*
1. Anxiety	10	2	5	22
2. Efficiency	1	12	5	..
3. Responsibility	2	1-	3	..
4. Life goals	4	2	4	..
5. Methods	16	9	17	7
6. Consultation	1	3	1-	..
7. Occupation	4	5	2	..
8. Persuasion	5	2	1-	..
9. Philosophy	5	2	8	..
10. Pleasure	4	4	2	14
11. Psychosomatics	4	4	2	..
12. Recreation	1-	1	2	..
13. Relaxation	5	..	1-	..
14. Self-confidence	4	12	5	..
15. Self-diagnosis	2	1-	4	..
16. Self-tolerance	4	1	2	..
17. Self-understanding	5	4	9	..
18. Sex and marriage	5	19	22	50
19. Sleep	3	1	1-	7
20. Social	15	16	6	..
Inspirational	1	1	1-	..

* Decimals eliminated. Figures represent nearest whole numbers.
† 1- indicates less than 1%.

an unmistakably inspirational, "pep talk" nature. The twenty-first item, therefore, is not a separate category, but a description of the manner in which directives were given:

Inspirational. "Put your *will* to work; don't falter and you can't fail! All you have to do is keep at it long enough"[1, p. 223].

Results

The proportion of directives devoted to each topic was calculated for each unit sampled, and on that basis the average proportion for each type of literature was established. Table 1 summarizes the latter findings.

Analysis of Statistical Findings

1. When three samplings of literature are grouped (Columns 2, 3, and 4) the four topics with the *highest* per cent of directives are: sex and marriage, methods, anxiety, and social. The *lowest* per cent is devoted to: recreation, consultation, sleep, and—with identical scores—responsibility and relaxation.* Taken collectively, stress is on two of the most significant practical human problems, sex and social adjustment. Remarkably little is placed on the third area, that of vocational satisfaction.
2. Topics in each of the *top* halves of Columns 2, 3, and 4 are: sex, methods, self-confidence. Topics in each of the lower halves of these columns are: responsibility, recreation, self-tolerance, sleep, and consultation.
3. When popular books and popular journals are grouped and compared with academic books, it is seen that sex, methods, and self-confidence received the *most* attention in both instances. Responsibility, sleep, recreation, consultation, and self-tolerance received the *least* attention in both groups.
4. With one exception all three categories of literature deal at least to some extent with each of the 20 topics. (The exception is "relaxation," which—perhaps by chance in the sampling—had no directive devoted to it in the popular journals). Popular and academic advice, *when many literary units are grouped,* is quite similar in its emphasis and wide in its coverage. It should be noted that these summary results do not reach the reader of the single unit, whose impression depends more on factors referred to in "5."
5. Single books and journals are characterized by enormous variability in the per cent of directives on each topic. One selected illustration is the fact that although sex and marriage is the highest ranking topic in every regard, three popular books gave no advice whatsoever on the subject[5, 7, 9]. On the other hand a selected example of individual emphasis in a single unit is Dunlap's[13] concern with directives on only 8 of the 20 topics, with about 65% on sex and marriage. The designation "mental hygiene," or any similar term, permits almost no conclusion about what the reader will find in a particular book or journal. It might be speculated that his confusion about how much advice he needed, and what sort, would increase directly with the number of books and journals he reads.
6. Inspirational advice of the lift-yourself-by-your-bootstraps variety, although it ranks highest in the popular books, is almost non-existent in any category studied. Directives on "consultation," the possible need for personal attention, are more concentrated in popular than in academic units. Finally, academic books give more advice on self-diagnosis. These three findings might run counter to the untested beliefs of many psychologists.
7. Academic books have notably fewer directives on "social" and "occupation," than are found in popular writing. In this sense they may underestimate their interest value and practical significance to the usual reader.

* The four texts on abnormal psychology are included primarily for interest and contrast; their total number of directives, only 10, is omitted in this summary.

Conclusions

Analysis of the directives in three samplings of mental hygiene literature leads to these main findings:

1. Popular and academic literature on mental hygiene, *as a whole*, is consistent to a considerable extent with respect to most-stressed and least-stressed topics.

2. *Single units* of mental hygiene literature, whether popular or academic, are distinctly individual products and the topics on which they concentrate advice varies greatly.

3. Facile assumptions about differences between popular and academic categories may not only be false but reversed.

4. A survey of mental hygiene literature leaves a marked impression of arbitrary emphasis and approach in each unit. It is submitted that the dignity of the field as applied science, as well as public confidence in it, would be increased by greater agreement among its writing practitioners.

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ALTITUDE AS A REFERENCE POINT IN SCATTER ANALYSIS*

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Introduction

A number of measures have been proposed as internal reference points from which an assessment of intra-individual test discrepancies can be made. Babcock[1] and Rapaport[9] have made extensive use of the vocabulary test for this purpose. Their rationale stresses two points: one, that vocabulary is relatively unaffected by mental disorganization, and two, that vocabulary is a satisfactory measure of intelligence. Criticism against the use of vocabulary as a special measure of intelligence is premised on the fact that since vocabulary is a verbal test, the intelligence level of non-verbally minded individuals is under-estimated [3, 12]. The continual emergence of verbal ability as a group factor[13] also speaks against the acceptance of vocabulary as a sole measure of intellectual potentiality.

A central tendency score is a second type of widely used reference measure. In their psychometric patterning, Magaret[7] and Wechsler[12] have compared the differences of each individual's sub-tests from the mean of all scores in clinical and control populations. Rapaport[9] also utilizes deviations from a modified verbal mean and from the performance mean. Because they are the results of averaging, means serve as fairly stable references. The gain in statistical stability, however, is offset by the psychological ambiguity of such a score. The averaging of unhomogeneous measures has been criticized by Babcock[1] and Jastak[6]. Thurstone[11] has abandoned the practice in favor of presenting a number of scores, each having its own discrete psychological meaning.

More recently, Jastak[3, 5, 6] has introduced the concept of altitude as an aid to scatter analysis. By altitude, he refers to the top scores in an individual's test battery. On both theoretical and statistical grounds, he conceives of altitude as the numerically derived representative of native intellectual capacity and the clinical correlate of Spearman's G factor. Intelligence, he defines as "the level of maximum personality integration," asserting that the optimum and not the medium is best indicative of the individual's potentialities. Wide deviations between capacities and functional ability in life signalize maladjustments, as large discrepancies between altitude and selective scores on psychometric batteries signalize disorganization. Hence the rationale for utilizing the highest scores as a reference point for scatter analysis.

The problem of selecting a reference point is not merely one of methodological technique, but one that bears directly upon the broader problems of the nature of intelligence and personality organization. The three types of reference points discussed actually stand for three different measures of intelligence which, while current in clinical practice, conflict with one another at various methodological and theoretical levels. For example, mean and altitude proponents accept Spearman's G factor, but disagree on what measure best represents it. However, the choice of a measure depends on the chooser's conception of what this G factor is. If it is conceived as an ability, then the I.Q. is used. If conceived as capacity, then altitude is favored. Vocabulary and altitude proponents both stress intelligence as potentiality and consider test scores falling significantly below this potentiality as representative of mental inefficiency[1] or adverse personality traits[6]. There is disagreement over the procedures to be used in assessing this potentiality, but the validity of these procedures hinges upon a clear-cut demonstration of their relative value in differentiating levels of intelligence independent of functional efficiency. As Chein[2] puts it, "to bring back the concept of capacity to the realm of science, ... there must be some means of diagnosing a capacity independently of its

* The author thanks Dr. Joseph Jastak, Chief Psychologist at the Delaware State Hospital, for his helpful suggestions for the preparation of this manuscript for publication.

TABLE 1. *The ages, educational levels and Wechsler-Bellevue scores for the control and experimental groups*

	N	*Schooling*	*I.Q.*	*Verbal*	*Performance*
Nurses	50	12.9 grades	105	104	105
Patients	50	10.8 grades	79	83	77

being fulfilled." Furthermore, the very selection of any particular reference point determines to a great extent the ensuing pattern of intra-test variability from which the clinician attempts to draw diagnostic inferences or personality pictures. Finally, in research work, the use of a standard vantage point that is practicable, statistically stable, and psychologically meaningful is indispensable for a direct comparison of scatter studies.

The purpose of this paper is to investigate the feasibility of using altitude as a reference point in the derivation of scatter patterns.

METHOD

Population. Our control group consists of 50 white females who applied for nurse's training at the Delaware State Hospital. The experimental group comprises 50 white female patients diagnosed as schizophrenic at the same institution. Table 1 summarizes the age, educational level, and Wechsler-Bellevue I.Q.'s of these two groups.

Scoring. The weighted scores of the Wechsler-Bellevue sub-tests of each individual were transmuted into quotients by the following procedure. Each weighted verbal score was multiplied by 5, since there are five tests in the verbal cluster. The obtained result actually expresses the contribution this particular test score makes to the total weighted verbal score. The corresponding quotient is then found in the usual manner in Wechsler's tables of verbal quotients under the individual's age. Performance weighted scores undergo the same procedure, only the final quotient is found under the table of performance quotients. This method, devised by Jastak, enables us to control to a great extent the fluctuations of individual sub-test scores occasioned by age differences between individual cases.

Quotients from Jastak's Wide Range Reading Test[4] supplemented the standard Wechsler-Bellevue battery. Twelve quotients were found for each case, eleven from the sub-tests of the Wechsler-Bellevue, the twelfth being the reading quotient. To overcome the relative instability of single scores, altitude for each case was computed by averaging the three highest quotients in the individual battery. In order to give a proportionately heavier weight to those scores at the ceiling of the individual's achievement, the highest score was weighted 5, the next highest 3, and the third highest 2. Each test quotient was then divided by its respective altitude to yield a deviation ratio. The use

TABLE 2. *Deviation ratios from altitude of 50 nurses and 50 female schizophrenics*

	Nurses		*Schiz.*		
	Mean Altitude 123		*104*		
	Mean	*S.D.*	*Mean*	*S.D.*	*t*
1. Vocabulary	86.8	7.20	94.1	7.98	4.78
2. Information	88.3	9.93	88.2	9.63	.03
3. Comprehension	88.1	9.60	79.0	12.20	4.13
4. Similarities	85.7	7.34	82.0	11.88	1.74
5. Digit Span	81.6	11.31	79.4	12.93	.89
6. Arithmetic	80.0	15.05	71.3	14.90	2.88
7. Picture Arrangement	81.0	12.50	73.6	13.73	2.77
8. Picture Completion	81.8	11.55	65.9	16.26	5.58
9. Block Design	89.0	10.62	80.2	14.07	3.57
10. Object Assembly	86.2	11.95	74.5	19.68	3.56
11. Digit Symbol	95.5	9.12	76.2	15.33	7.57
12. Reading	88.6	9.00	95.7	11.30	3.45

of ratios instead of differences serves to keep altitude uncorrelated with the test deviation and thus acts as a control upon mental age.

Selective factors such as age, education, and choice of profession contribute to making the nurse's group more homogeneous in composition than the schizophrenic group. In a clinical setting, it is quite difficult to obtain a random sampling of normals to serve as a control population, which is why attendants, patrolmen, and nurses are so frequently utilized. However, since we are interested more in the nature of altitude as a scatter tool and as an intelligence measure, rather than in obtaining actual scatter norms, and since the nurses are undoubtedly a more stable group than the schizophrenics, which factor should yield differential scatter patterns, it is believed that the choice of groups is justified by the direction and scope of the problem.

Results

A comparison between the deviation ratios of schizophrenics and nurse applicants is presented in Table 2.

The vocabulary and reading deviation ratios of the schizophrenics are reliably higher than the corresponding deviation scores of the nurses. Those tests whose deviation ratios are significantly depressed among the schizophrenics are comprehension, arithmetic, picture arrangement, picture completion, block design, object assembly and digit symbol. The similarities test shows a trend toward impairment among the patients, though the critical ratio fails to reach the 5% level of significance.

To gain an estimate of the relative stability of each of the different reference points in use, the significance of the differences between the standard deviations of altitude, vocabulary, the total I.Q., the performance quotient, and the verbal quotient were computed. None of the obtained differences were significant.

In order to compare the relative effectiveness of altitude and vocabulary as measures of intelligence, their correlations with total I.Q. are presented in Table 3.

In both groups, altitude correlates more highly with I.Q. than does vocabulary.

Table 3. *Correlations between I.Q., Vocabulary, and Altitude*

	Nurses		*Schizophrenics*	
	r	*S.D.*	*r*	*S.D.*
I.Q.—Vocabulary	.45	.114	.82	.047
I.Q.—Altitude	.83	.044	.91	.027
Altitude—Vocabulary .	.43	.116	.89	.028

The higher correlations with I.Q. of altitude is signficant at the 1% level (C.R.: 3.00) even in the schizophrenic group.

Discussion

The significant pattern deviations from altitude point to the latter's practicability as a reference measure. Since there have been no previous studies reporting altitude scatter on the Wechsler-Bellevue among psychotics, opportunity for direct comparison of the above scatter patterns with those submitted by other investigators is limited.

The fact that no reliable sigma differences appear among the various reference measures in either schizophrenic or normal samplings suggests that altitude is fully comparable to any of the extant reference scores in its limited range of variability within the groups studied. Variability is both an inter-individual and an intra-individual concept. The above results attest to the inter-individual stability of altitude. Equally pertinent, is the question of its intra-individual variability. The thesis that altitude is a valid measure of native capacity and not merely another convenient operational statistic depends in part upon the demonstration of its intra-individual constancy. This cannot be demonstrated in this study since sufficient retest data are not available. It has been the clinical experience of Jastak, who has devised the measure, and of the writer, who has used it, that the altitude quotient is relatively impervious to either deterioration or improvement, even when successive testing discloses a loss or increment in the individual's global I.Q. However, validity studies attacking this problem remain to be done.

The inter-correlations between altitude, vocabulary, and I.Q. raise interesting points. In both nurses and schizophrenics, altitude correlates highly with

I.Q., and significantly more highly than does vocabulary. Here we have a measure whose absolute height is relatively uncontaminated by the factors of functional efficiency implicit in the I.Q., yet whose directional covariation coincides even more closely than does vocabulary with an index of mental ability. These findings tend to support Jastak's conception of altitude as a diagnostic measure of mental potentiality relatively free of disorganizational factors, and furnish statistical evidence for its rationale as an effective reference point. The question of the relative validity for the measurement of intelligence by I.Q. as opposed to altitude is an important one in clinical practice. The correlation coefficient measures correspondence of directional variation rather than correspondence of absolute magnitude. Since the clinical assignment of an intelligence level is based on the magnitude of the diagnostic measure, intelligence estimates are critically affected by the psychometric criterion utilized. For example, the average altitude score in the schizophrenic group is 104, whereas the average I. Q. is 79. It becomes obvious then, that even though the two measures, altitude and I.Q., are highly correlated, the very real magnitude discrepancies between them raise a serious diagnostic issue—which score best represents intelligence level? We can draw one inference regarding the relative worth of I.Q. and altitude as intelligence measures which can be put to a test. If altitude measures intellectual potentiality, and I.Q. is a measure of functional efficiency, then the downward deviation of I.Q. from altitude should be greater in a disorganized group than in a relatively stable population. This is indeed the case, for the average deviation ratio of I.Q. from altitude in the nurse's group is 85.26, S.D. = 6.46, while in the schizophrenic group, the average deviation ratio of I.Q. from altitude is only 75.40 with an S.D. of 9.12. The difference is extremely significant (C.R. = 6.26). It may be similarly argued that the discrepancy between vocabulary and I.Q. would be greater in the schizophrenic group than in the nurses, and that since vocabulary is relatively higher than I.Q. in the disorganized group than in the normal one, a point is made for vocabulary as a measure of intellectual potentiality. However, 38% of the nurses actually obtained I.Q.'s which were of greater magnitude than their vocabulary scores. It becomes difficult to conceive of a potentiality or a capacity which is smaller than its fulfillment. Altitude is always larger than I.Q., and in this sense fulfills an elementary requirement of the concept of potentiality if the latter is to have any consistent meaning, namely that potentiality be greater than actuality.

The relatively low correlation between I. Q. and vocabulary in the nurses is undoubtedly due to the strong homogeneity of this group. In larger, more heterogeneous samplings, it is appreciably higher[8, 10, 12]. The high correlation between vocabulary and altitude in the schizophrenic group suggests that much of the invulnerability of vocabulary among psychotics is due to its functioning as an altitude surrogate in psychotically disorganized people. In non-verbal, psychopathic, and specially selected groups, the correlation between altitude and vocabulary is probably much lower. The lower correlation between the two measures in our selected normal group is in line with this supposition. It should be borne in mind that the very construction of the vocabulary test, apart from its validity or invalidity as a measure of intelligence, contributes to its high correlative properties. It contains a large number of carefully graded items; fine units of scoring are employed; and its wide range of difficulty facilitates the tapping of correspondingly wide ranges of intellectual level. If other types of tests are to be compared with vocabulary as indices of intelligence, they should first be made equally reliable as technical tools. It is likely that all equally reliable tests correlate equally well with altitude in random samplings, though which tests emerge as altitude measures within specific individuals or discrete groups depends on those traits dominant within those individuals or groups.

Summary and Conclusions

In order to assess the value of altitude as a reference point, the Wechsler-Bellevue sub-test deviations from altitude of 50 schizophrenics and 50 nurse applicants

were compared. Significant differences were obtained. The stability, invulnerability, and significance of altitude as a measure of intelligence were compared with those of the standard reference indices.

It is concluded that within the limits of the size and selectivity of our experimental populations, altitude offers promise of being a practicable, psychologically meaningful, and experimentally fruitful vantage point for the derivation of scatter patterns.

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THE FULL-RANGE PICTURE VOCABULARY TEST: IV. RESULTS FOR A WHITE SCHOOL POPULATION[1]

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INTRODUCTION

General verbal ability is closely related to general intelligence. For this reason, intelligence tests have almost always included an appreciable number of verbal items. The two most widely used clinical tests of general intelligence at the school level are the Revised Stanford-Binet[10] and the Wechsler - Bellevue[12]. Each makes important use of verbal items. Unfortunately for many uses the Stanford-Binet calls for reading at Year 10, and takes approximately an hour to administer. In addition, the variety of tests often makes interpretation of the pattern of "passes" difficult in terms of the subject's possible ability or experience. The Wechsler's usefulness is limited in that it should probably not be used regularly at ages below 12, and its standardization group for the upper part of the school range was not directly controlled for socioeconomic status.

Various kinds of vocabulary questions have proven to give better measures of verbal ability than any other sort of verbal item. Both Stanford-Binet and Wechsler vocabulary subtests show the highest correlation of any subtest in the scales with the whole scale scores. Spache[9] has independently determined

1. Acknowledgment is due Mr. Neil W. Coppinger, Mrs. Helen S. Ammons, and Mr. Newell H. Berthelot-Berry of Tulane University for reading the manuscript critically and offering many helpful suggestions. The test and manual with final scale norms, answer sheets, and instructions for administration[1] can be obtained from R. B. Ammons.

the correlation between the MA's from the Stanford-Binet vocabulary test and the full scale MA's to be .92.

No really satisfactory scale using only vocabulary items has as yet been constructed and standardized. Such a test should not depend on reading ability or a facile flow of speech. It should correlate well with measures of general ability, and take only a short time to give. With these considerations in mind, the senior author considered a variety of vocabulary items, and concluded that the multiple-choice picture vocabulary form used by Van Alstyne[11] was the most promising. A set of 16 4-picture plates was developed, and a preliminary scale seemed to work well in practice[4]. On this basis a program was set up for developing and standardizing a scale for use at all age levels[3, 6, 7] and with special groups, such as farm[5] and Spanish-American[2] children.

Problem

The purpose of this study was to develop two forms of a picture vocabulary test using the 16 4-picture plates provided by Ammons and Huth[4], and suitable for measuring the auditory comprehension vocabulary of school children 6 to 17 years of age. To set up the final forms, the following steps were necessary: (a) discovery of a large number of potentially useful items, (b) testing of a representative school-age population, (c) selection of items for the two final forms of the test, (d) evaluation of these forms as to reliability and validity, and (e) calculation of norms for future use based on the scores made by the standardization group on the items making up the final forms.

Procedure

Materials. The 1937 Stanford-Binet vocabulary words[10] were listed on a special form allowing enough space to record answers in full. In giving the Full-Range Picture Vocabulary Test, the 16 4-picture plates developed by Ammons and Huth[4] were used. The pictures are cartoon-like line drawings representing a considerable variety of life situations.

A large number of items related to the pictures was needed. The concept of each item was supposed to be represented by only one picture of the four on a given plate. Forty-eight items were available from a previous study[4], and the standardization personnel were able to list 243 more by examining the pictures and working through a dictionary. Group discussion eliminated 43 of these, and the remaining 248 were roughly pretested by administering them to 48 white children,[2] 2 boys and 2 girls from each grade, 1 through 12, which children had been judged to be of about average ability by their teachers.

Passes for each word at each age level were tabulated. A running average based on five age groups at a time was calculated. From this a 50 per cent passing point was estimated for each word, *i.e.*, the age in the age succession at which approximately 50 per cent of a group of children would be expected to pass. Since the vocabulary test of the 1937 Stanford-Binet had also been given, the difficulty of the items could then be expressed in terms of the estimated MA level corresponding to the 50 per cent passing point.

On the basis of the number passing in each age group 22 more items were discarded, leaving 226 for the standardization scale. An item was considered inadequate if it discriminated poorly between successive age levels, showed sex differences in difficulty, or was defective in some qualitative way. A testing form was arranged with the whole series listed in order of difficulty by card. (Of the 226 items used in the standardization testing, 157 had estimated difficulty levels in the age range from 6 to 17 years.) The total list is given in an article by Ammons and Rachiele[6], where a more detailed

2. The authors were fortunate in having the help of the following Denver public and parochial school personnel in obtaining children for pretesting and standardization: Claude B. Pendleton, Roy Hinderman, Harry Nicholson, Maurice Ahrens, Guy Fox, Joseph M. Lort, Eugenia M. Kaupp, George E. Hook, William L. Miller, Margaret McGinley Murray, Alvin Yordy, Rev. Edward A. Leyden, Sister Anna Joseph, Peggy Chambers, Sister Mary Owen, Sister Mary Salome, F. Allan Murphy, and Robert Colwell. Without their generously given assistance, this project could never have been undertaken, much less completed.

TABLE 1. *Number of subjects of each sex tested at each age level, with mean chronological age in years for each group*

	Males		*Females*		*Total*	
Age group	*N*	*Mean CA*	*N*	*Mean CA*	*N*	*Mean CA*
6	11	6.5	13	6.6	24	6.6
7	8	7.3	16	7.4	24	7.4
8	21	8.4	12	8.5	33	8.5
9	15	9.5	11	9.5	26	9.5
10	17	10.5	19	10.5	36	10.5
11	10	11.4	16	11.5	26	11.4
12	16	12.4	16	12.4	32	12.4
13	20	13.5	14	13.4	34	13.5
14	9	14.5	12	14.3	21	14.4
15	19	15.5	17	15.5	36	15.5
16	11	16.4	15	16.4	26	16.4
17	16	17.4	15	17.5	31	17.5
Total	173		176		349	

explanation of the item selection method is also presented.

Subjects. A total of 360 children were tested, but the 11 at the 18-year level were discarded because it was impossible to keep the sampling at this school level unbiased according to the sampling criteria. The age distribution of the remainder of the group is shown in Table 1. Fifteen boys and 15 girls were tested at each grade level.

Only those children considered by the school to be in the proper grade for their age group were taken. This grade level control led to the unequal numbers by ages shown in Table 1.

Father's occupation was carefully controlled by grade, as shown in Table 2, in the same way as described by Ammons and Holmes[3] for the pre-school-age group. It will be seen that there is a fairly close correspondence in occupation between this group of fathers and employed white males in the United States who are heads of families. The farm children in this study (3 boys and 3 girls at each grade level) were tested separately[5], but their test results are used to complete the sample. Children with no fathers, unemployed fathers, or fathers with unreported occupations were excluded from the sampling. All the city children were taken from the public or parochial schools of Denver, while the farm children were tested in a rural area in Nebraska[5].

TABLE 2. *Percentage of present school-age population tested with fathers falling in certain major occupational categories*

*Occupational group of father**	*Per cent tested*	*Estimated census per cent†*
Professional and semi-professional	5.8	6.5
Farmers and farm managers	18.6	18.2
Proprietors, managers and officials	11.7	11.8
Clerical, sales, and kindred workers	11.4	12.3
Craftsmen, foremen, and kindred workers	17.8	15.5
Operatives and kindred workers	20.0	19.0
Service workers	4.4	6.3
Farm laborers and foremen	3.1	3.3
Laborers, except farm and mine	7.2	7.2
Total	100.0	100.1

* Classification taken from the 1940 census (13a).

† Computed from the 1940 census (13b), Table 16, families with employed head, by major occupation group of head, by tenure, for the United States.

Testing. Children were tested during school hours in special rooms provided by the cooperating schools. Once suitable rapport had been established by conversation, the picture vocabulary testing was begun. Words were asked for one plate at a time, and always for the plates in the same order, from 1 to 16.

Since all people in the standardization project[3, 4, 5, 6, 7] were expected to administer some tests at each general age level, a standard set of instructions was prepared[1]. The subject was told that all he had to do was to point at the one picture out of four on the plate before him which the word described. The procedure was then tried out with several simple words, until it was clearly understood. It was explained that there were no time limits. Guessing was not allowed, and the subject was instructed to say "I don't know," if he didn't. It was explained that there were words for adults on the cards, so that there would be many he could not give an answer for.

Easy words were asked whenever the examiner thought this would raise morale and prevent guessing. If a subject seemed to hesitate before pointing, or give any other indication of guessing, he was asked why he had chosen the picture he had, asked to define the word verbally, or the item was asked again later as a check. These procedures seemed to eliminate guessing in all but a few cases. Evidence for this can be found in the numbers passing by ages for the various items, where the numbers somewhat below the 50 per cent passing point were zero or nearly so, whereas 25 per cent of the subjects would have answered correctly by chance if all had been guessing. Starting at the subject's estimated level of ability, words were given until three consecutive levels had been failed and three passed.

When the picture vocabulary test had been completed, the 1937 Stanford-Binet vocabulary test was administered, according to standard instructions[10].

Results

Responses were tabulated by items and age groups, and the per cent passing at each age level was calculated for each item. A 50 per cent passing point was determined by interpolation between the two adjacent age levels which showed per cents passing above and below 50 respectively. In tabulation, all items below the three levels consecutively passed by a given subject on a plate were scored passed; and all items above the three consecutively failed were scored failed.

Selection of the final items was now made. It had been decided to keep 8 words at each age, 6 to 16, or a total of 88 out of the original 157 available. An item analysis was done to eliminate the words which most poorly discriminated between successive age levels, those showing differences in difficulty between the sexes at beyond the 5 per cent level of confidence by a chi-square test, several which referred to more than one picture on a plate, those with a regional bias, some which showed more subjects passing at lower age levels than at higher, and some which duplicated concepts better tested by the same or a similar word on another plate[6]. No word was kept which had been found to be inadequate at the preschool[3] or adult[7] level.

It was now discovered that there were few than 8 satisfactory items at the 8-, 11-, 12-, 14-, and 15-year levels. This was remedied by borrowing from adjacent levels with a surplus. This actually made little difference in the average difficulty of items at a given level, since only items close to that level were borrowed. For example, since only 7 words were available at the 8-year level, "sale" at the 7.9 year level was included to fill out this level. All items remaining beyond 8 at a level were eliminated in order of their unacceptability according to the previously mentioned selection criteria. Although the 17-year level was not included in the final scale, two words from it were borrowed to fill out the 16-year level. It was found that adult level 3 was equivalent to the 17-year level, so that when adult levels 1 and 2 were omitted, the items could be ranged on a continuous scale of difficulty, 14, 15, 16, A3, A4, etc.

Two forms, A and B, were now constructed, by taking four words at a time from the list by difficulty, and assigning

them to the scale according to the scheme ABBA. The final items are given in the test manual[1].

Each subject's score was determined on each final form by counting the items passed, including those at the preschool and adult levels, and all those below the three-consecutive-pass level on each plate. The correlations of picture vocabulary with Binet vocabulary scores were computed separately by forms and age groups. These were quite variable, due to the small numbers involved, but showed medians of .67 for Form A and .69 for Form B, with no systematic sex difference. It was felt that these might be attenuated due to the unreliability of the criterion, the Binet vocabulary score. Consequently the odd-even reliabilities corrected to full length[8] of the Binet vocabulary were computed separately by age groups. These ranged from .50 to .91, with a median of .80. The obtained validities for the picture vocabulary test based on its correlations with the Binet were thus reasonably high, and corrected for the attenuation of the criterion, they were very high. The odd-even reliabilities of the picture vocabulary test varied from .50 to .99 with a median of .81. The reliability for the full range of talent was .987. Here again there were no significant form or sex differences.

Table 3 gives the mean scores by form, age-level, and sex.

There were no significant differences between sexes or forms. The steady progression from age to age is another indication of the validity of the test. Final norms[1] were prepared by smoothing the values given in Table 3.

The mean Binet vocabulary scores of the present sample and those estimated[4] for comparable age levels in the original Terman - Merrill standardization group were compared. There is a slight but statistically unreliable tendency for the present group to score higher, but this is probably due to the method of estimating original Binet vocabulary scores which was rather rough[4].

Discussion

Certain limitations of the present sampling procedure are apparent. Since only children who were in the correct grade for their age were tested, the upper and lower extremes of ability were excluded, and the group variability in picture vocabulary scores was decreased. Group variability in scores was increased by including a variety of ages within any age group, *i.e.*, age 7 could include children 7 years 0 months to 7 years 11 months. The group was taken from only two localities, and to the extent that the children of persons engaged in specific occupations in Denver and a farm area in Nebraska are not representative of those whose parents are engaged in the same occupations elsewhere in the country, the present sample is not representative.

These limitations are not serious. As

Table 3. *Mean scores for Form A and Form B of the Full-range Picture Vocabulary Test according to age level and sex for the present school-age standardization group*

	Form A				*Form B*			
	Male		*Female*		*Male*		*Female*	
CA level	*Mean**	*S. D.*†	*Mean*	*S. D.*	*Mean*	*S. D.*	*Mean*	*S. D.*
6	24.7	4.2	25.3	5.0	25.4	3.9	25.8	5.6
7	28.5	4.4	29.2	4.8	28.4	3.2	29.8	5.4
8	33.2	5.2	29.9	4.3	33.8	7.9	29.9	3.9
9	36.2	6.3	34.9	6.5	38.4	6.4	35.2	6.2
10	42.8	5.3	41.7	4.9	41.3	6.1	41.1	7.7
11	52.8	5.3	43.2	6.6	51.0	4.3	43.4	6.5
12	53.5	6.9	47.8	5.7	53.5	6.6	46.4	8.6
13	55.7	5.5	50.3	6.0	53.5	6.2	51.6	6.1
14	58.4	5.5	57.8	5.2	57.8	6.4	57.5	7.7
15	63.7	6.3	59.3	4.4	62.8	7.1	60.2	6.5
16	63.7	6.1	62.5	5.8	62.7	7.2	63.1	7.5

* Mean score.
† Standard deviation.

pointed out elsewhere[7], the representativeness of a sample is only relative, as are the norms based on it. The present group is certainly representative of a large proportion of the white school-age population of the United States. It might be pointed out that Wechsler's sample of school-age individuals was taken from a limited number of schools in New York City, and is certainly of an indeterminate representativeness. In any case, the actual validity of the present test will rest in the last analysis on clinical experience. It is interesting to children, easy to administer, and reliable in its estimates of verbal ability. It should be mentioned that the pictures can be used to obtain personality information by questioning the child's spontaneous expressions of interest and emotion or asking him to tell stories.

Summary and Conclusions

A preliminary scale of 226 words based on the plates of the Full-Range Picture Vocabulary Test was administered with the Revised Stanford-Binet vocabulary test to 360 white children, 15 boys and 15 girls at each grade level from 1 to 12. The sample was controlled for the occupations of fathers. A set of 88 items was selected on the basis of this group's performance, divided into two forms, and combined with items from the preschool and adult levels to make up the final Full-Range Picture Vocabulary Test. Reliability based on intercorrelation of the forms was .987 for the whole group, and the forms correlated well (.67 and .69 uncorrected for the unreliability of the criterion) with the 1937 Stanford-Binet vocabulary test. Norms are given for ages 6 to 16.

In view of the reliability and validity of the test, its high interest value and short time of administration, and its provision for a simple non-verbal way for the child to answer, it should prove valuable in the field of individual clinical measurement of verbal ability.

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THE BENDER GESTALT TEST AS A PROJECTIVE TECHNIQUE

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Introduction

Since its publication twelve years ago, the Bender Gestalt Test has received increasing attention as a projective medium. In 1938, Bender wrote "The final product (of copying forms) is a visual motor pattern which reveals modifications in the original pattern by the integrating mechanism of the individual who has experienced it"[3]. Since then, the problems concerning the application of the test have been resolved as follows[4]:

1. Establishment of an objective scoring scheme.
2. Extraction of the meaningful factors from the final results.
3. Relation of the above to the personality organization of the individual.

The purpose of this paper is to offer a solution to the first two of these. A scoring method is proposed, the results of which are correlated to various factors obtained from the Rorschach test.

The history of the use and application of the Bender test has been amply reported by Billingslea[4]. A summary of the methodology and results obtained in these studies, is, however, of some interest.

1. Some attempt was made to separate and classify meaningful items of performance.
2. The categories thus obtained were both qualitative and subjective in nature.
3. The question of reliability was not investigated.
4. Validity was considered only in terms of the differential classification of clinical groups such as neurotics versus normals, psychotics versus neurotics, etc.
5. The results of the validity studies were questionable at best.

Billingslea's own approach represents, in part, a radical departure from the preceding investigations. Following an analysis of the conceptual foundation of the test he devised and developed an objective method of scoring based upon an elaborate system of factors and indices. Three of the factors, closure, total rotation, and size difference, showed evidence of statistical reliability via the split half technique. Validation, by a comparison of the results made by groups of normals and selected neurotics was, in general, unsuccessful[4].

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Theoretical Considerations

In order to consider adequately the problems of scoring and validation it is necessary to refer to some of the theory underlying projective techniques and personality dynamics. According to the most generally accepted concept of personality a continuum is presumed to exist between the "most normal" and the "most abnormal"[8]. The nosological categories of clinical diagnosis represent an arbitrary classification system which may or may not be justified in fact. Certainly, it is unwarranted to assume that a test is capable of making such distinctions in vacuo[18]. It is more logical to expect that a projective technique yields evidence of the personality structure by means of behavioral manifestations, in one form or another. The clinician supplies the "label" —if and when indicated. Furthermore, within each category, *e.g.,* schizophrenia, there exists great diversity in individual personality organization. Since test results vary accordingly, the evaluation of the meaningfulness of individual factors within their inter-relationships become as important as the consideration of the total performance[18].

In general, physical and psychological events may be understood either as convergent or divergent phenomena according to the conceptual formulation and methodology employed[6]. The former may be called the psychometric approach. It studies the regularities of behavior in terms of individual differences, establishes central tendencies and group norms, and utilizes classical statistical methods in

order to develop group relationships. The latter point of view is inherent in most modern personality theory and in our understanding of projective methods. Personality is considered to be an active, dynamic, stratified, and structured whole which interacts with its environment[14]. Any facet of this interaction reveals and reflects the whole but may be interpreted accurately *only* in light of knowledge of the entire personality[2]. Stated in terms of the immediate problem, the validation of data obtained from projective tests should be attempted through integration with all the data available on the individual personality, rather than through statistical treatment of group results[6].

The introductory phase of the application of a testing method does require postulation of a classificatory scheme, and correlation with known variables. But, it must be remembered, such a breakdown is artifactual and is useful only for the determination and extraction of meaningful material. The final check of validity can be obtained only by following the criteria discussed above[6].

Method

Administration. The standard Bender-Gestalt figures supplied by the *American Orthopsychiatric Association* were used. A pencil with an eraser and a supply of white, unlined 8½" by 11" paper were presented to the subject, with the following instructions: "I am going to show you a series of diagrams, one at a time. What you are to do is to copy these diagrams just as you see them on the cards"[3]. The first card was then exposed, followed by the second when the subject completed the first to his satisfaction, etc., until the test was completed. All questions were answered either by, "draw them just as you see them," or by "do what you think best." No drawing aids of any kind were permitted.

Immediately after the completion of the Bender Test, each subject was given a Rorschach, according to the usual methods for individual administration. Each record was scored by the method outlined by Klopfer and Kelley[10].

The subjects consisted of a group of sixty college undergraduates who had volunteered for an extended program of testing[5].

Scoring Method. Size deviations from the stimulus figures were measured according to the twenty-five indices listed in Table 1. The unit of measurement chosen was 0.1 inch since it was large enough to allow for coarseness in drawing and since the dimensions of the stimulus figures were found to be integral multiples of this unit. Graph paper ruled ten units to the inch was found to be a very convenient measuring device. Standard scores were computed for the deviations and the standard deviation (V score) and algebraic total (D score) were calculated on the basis of these for each of the subjects.

Results

The intercorrelations of both the V and D scores with selected Rorschach scores are presented in Table 2. It may be noted that statistically significant correlations were obtained with the V score and the following: sum C, .27; sum (CF + C), .28; M plus sum C, .34; M plus sum (CF + C), .37; and the "new F%," —.27. None of the correlations involving the D score was significant, but those which merit consideration are as follows: M, 0.23; M plus sum C, 0.22; W%, —0.24; and R, 0.22.

A reliability coefficient was obtained through the split-half technique by correlating the sum of the indices starred in Table 1 with the sum of those remaining. The resultant correlation was .85 and with the Spearman-Brown formula .92. The results, therefore, appear to be highly reliable. The correlation between the V and D scores was .38.

Discussion

Size variations in the Bender test may be evaluated in several ways. First, variation may be considered in terms of the direction of the deviation from the dimensions of the stimulus figures. The sum of the individual scores obtained from each index yields the overall tendency toward contraction or expansion for each subject. This is measured by the D score with positive quantities indicating expansion, and negative quantities contraction.

TABLE 1. *Indices for evaluating B-G responses*

Fig.	Index	Description of Index—Measured in Terms of Deviation from the Standard Figure	Stand. Fig. Dimension
a	I*	Vertical diameter of circle	1.0″
	II	Horizontal diameter of circle	1.0
	III*	Vertical diagonal of diamond	1.0
	IV	Horizontal diagonal of diamond	1.0
1	I	Length from first to last dot	5.2
2	I	Length of figure (horizontal)	5.1
	II*	Width of figure (vertical)	0.5
3	I	Length of figure (horizontal)	1.8
	II*	Width of figure (vertical)	1.1
4	I*	Vertical side of rectangular figure (mean)	0.7
	II	Horizontal side of rectangular figure	0.8
	III*	Altitude of curve	0.7
	IV	Distance between points of inflection of curve	0.9
5	I	Base of curve	1.1
	II*	Altitude of curve	0.8
6	I	Length of horizontal	5.0
	II*	Width of horizontal (mean)	0.4
	III*	Length of vertical	3.0
	IV	Width of vertical (mean)	0.3
7	I*	Length of left hand (tilted) figure	1.7
	II	Width of left hand (tilted) figure	0.6
	III*	Length of vertical figure	1.7
	IV	Width of vertical figure	0.6
8	I	Length of figure	3.0
	II*	Width of figure	0.6

Another measure of size variation is intra-individual variability in the actual performance. The size of the first figure drawn provides a pattern or frame of reference for the drawing of the second figure, the size of the first two for the third, and so forth. The standard deviation, or V score, represents this intra-individual variability within performance.

In the Rorschach, M is considered to be a manifestation of "promptings from within" and the sum C, responsiveness to stimulation from without[16]. At the highest level of abstraction, both the movement and color responses represent a reaction to aspects of the blot other than the obvious form elements[10, 15]. Conversely, the "new F%," a score proposed by Rapaport[15], is a measure of adherence to predominantly form elements. The significance of the color responses is heightened by substituting sum (CF + C) for sum C. The FC responses are more closely allied to the tendency represented by the "new *F%," and are, in fact, included within it.*

The success in discrimination of these tendencies by the V score may be found in Table 3. Two groups were selected, one composed of the five subjects with the highest V scores, and the other group, the five making the lowest scores. The average M plus sum C and the average "new F%" were calculated for each group, and the critical ratios were computed. The

TABLE 2. *Correlation between Rorschach and Bender scores*

Score	M	FC	sum (CF+C)	sum C	M plus sum (CF+C)	M plus sum C	W%	F%	New F%	R
V	.24	—.07	.28*	.27*	.37†	.34†	.01	—.07	—.27*	.04
D	.23	.02	.03	.10	.19	.22	—.24	—.03	.02	.22

N = 60.
* Significant at the 5% level.
† Significant at the 1% level.

differentiation of M plus sum C (r = .34) was significant at the 1% level of confidence. The "new F%" (r = —.27) was differentiated at the 5% level.

TABLE 3. *Critical ratios between high and low V scores*

Score	*High V*	*Low V*	*t*	*P*
M plus sum C	8	4.2	3.45	.01
new F%	77	93	2.54	.05

N = 10.

TABLE 4. *Critical ratios between high and low M plus sum C scores*

Score	*High M plus Sum C*	*Low M plus Sum C*	*t*	*P*
V	.8	.7	1.18	x
D	+6	—11	1.00	x

N = 10.

TABLE 5. *Critical ratios between high and low "new F%" scores*

Score	*High "new F%"*	*Low "new F%"*	*t*	*P*
V	.7	.9	1.18	x
D	20	23	0.5	x

N = 10.

Prediction of the V score by M plus sum C and by the "new F%" is shown in Tables 4 and 5 respectively. The groups were chosen by the same method used for the computation of Table 3. None of the critical ratios so obtained was significant. It is possible, therefore, to predict the Rorschach variables by the V score, but not vice versa.

Apparently, variability in performance is due, in part, to the interaction of the forces toward form adherence and to those against it. It is apparent, further, that the Rorschach does not measure all of the factors contributing to this variability. In general, however, it may be stated that the greater the V score, the greater the degree of distractibility from the "form aspects" of the task presented.

Since no significant correlations were found for the D score, discussion of its meaning must be confined to conjecture. It may be directly related to the productivity of the subject as indicated by R (r = .22). The M responses measure a distinct type of psychic activity—the interpretation of imbalance in the stimulus as movement[15]. Since M and the D score are positively correlated (r = .23), the tendency toward figure expansion could be related to this activity. The negative correlation with W% is certainly unexpected in light of our present understanding of the nature of W and merits more extensive investigation.

TABLE 6. *Critical ratios between high and low D scores*

Score	*High D*	*Low D*	*t*	*P*
M + sum C	7	4	1.50	x
"new F%"	82	88	0.80	x

N = 10.

TABLE 7. *Critical ratios between high and low (CF + C) and FC Scores*

Score	*Low (CF+C) High FC*	*High (CF+C) Low FC*	*t*	*P*
V	0.7	0.9	1.33	x
D	—10	+4	2.70	<.05

N = 10.

Discrimination of D and Rorschach scores by the selection of the highest and lowest groups was unsuccessful, as illustrated in Tables 4, 5, and 6. One exception was found by comparing the subjects who scored high in CF + C and low in FC with those who scored low in CF + C and high in FC (Table 7). The average of the former indicated expansion and that of the latter, contraction. The critical ratio was significant and well within the 5% level of confidence. Thus expansion appears to accompany uncontrolled affect and contraction, controlled affect. This relationship between figure size and emotionality is certainly not inconsistent with previous studies[9, 12, 20].

It must be emphasized that due to the limitations within the experimental procedure itself, many pertinent personality factors have remained completely unevaluated. The results obtained should not and do not represent evidence for or against

the validity of the Bender Test. They do reveal the presence of meaningful material which will require further study for the determination of its actual significance. The general method for such investigation has been discussed above. Immediate possibilities for further research include the development of a more sensitive scoring system, and use of more directly relevant criteria for validation.

Summary

1. An objective scoring method is proposed for the Bender Gestalt test based upon deviation of the results from the standard figures.
2. The scoring method was found to have high reliability.
3. The V score which measures intra-individual variability is related to the degree of adherence to, or departure from form elements.
4. The D score, representing tendencies toward expansion or contraction of the test figures, seems to indicate the relative control or lack of control of affect.
5. Theoretical problems involved in validation are discussed and suggestions are made for further study.

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THE BENDER VISUAL MOTOR GESTALT TEST AS A MEASURE OF SCHOOL READINESS

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Introduction

The classical *Gestalt-theorie* states that relationships are a secondary product of analysis of the primary total form inherent in sensory-perceptual activity, and that they are not fundamentally dependent upon either learning or broad experience. To this postulate, Bender [1] adds the concept that the sensory-perceptual-motor response is a personal function, dependent upon the maturational level and the integrative status of the organism. This exploratory study deals with the Bender postulate that the personal response is based upon the individual's maturational level. Specifically, it is an exploration of the corollary thesis that a child who is making satisfactory progress in reading must be at a maturational level where sensory-perceptual-motor activities more closely resemble those of adults than they do the responses of pre-readers.

Problem

The study reports the results obtained from the administration of the Bender Visual Motor Gestalt Test to two groups of normal children: the first group being 30 children in the Pennsylvania State College Nursery School who have not commenced to read; the second group being a random selection of 30 children in the second grade in State College, Pennsylvania. All the children were rated by the experienced teachers in charge as "normal children." One group had made satisfactory progress in reading; the other group was composed of pre-readers. The problem was that of finding out how these two groups differ in their reproductions of the nine Wertheimer figures comprising the Bender Test. As a tentative hypothesis, it was conjectured that the responses of the readers would more closely resemble those of adults than would the results obtained from pre-readers. In other words, it was supposed that there would be a significant difference when the reproductions of these two groups were compared. The results were first classified according to the categories outlined by Hutt[3].

Results

The findings are presented in table 1 below:

Table 1. *Percentage of distributions of responses on the Bender Visual Motor Gestalt Test for two groups of young children*

	Percentage of frequency	
Determinant	*Pre-readers N = 30*	*Readers N = 30*
Order:		
Logical	0%	73%
Confused	40%	0%
Irregular	60%	27%
Expansion	73%	40%
Use of margin	13%	58%
Size reduction	7%	86%
Condensation and simplification	100%	93%
Angulation	0%	42%
Curvature modifications	100%	81%
Perseverations	93%	58%
Closure effects	67%	97%
Overlapping	40%	3%
Elaboration	40%	0%

It appears that the pre-readers were entirely neglectful of the requirements of an orderly procedure in reproducing the figures and that incorrect sequences and chaos were characteristic of their responses. By contrast, the readers were cognizant of the need to proceed in orderly fashion, somewhat analogous to the methods followed by adults. Correlated with the relative disorderliness of the reproductions by the younger group was the tendency to spread the drawings haphazardly over the page in an expansive manner. The utilization of the margin to keep the figures well-spaced was neglected by most of the younger children in the pre-reading stage, whereas more than half of the second-graders were directed by marginal aspects of the field. It is also clear that the younger children were far less likely to reduce the size of the Gestal-

ten than the older ones were. Both groups tended to simplify the figures to make them easier to reproduce. This fact may be attributed to the imperfect co-ordinations of the children at these maturational levels. It is significant to report that none of the pre-readers followed the requirements of horizontal or vertical axes, but that nearly half of the older ones did. With this finding it could be predicted that there might be similar differences in imposing modifications of curvature. The facts, however, indicate that many children in the older group modified the curvatures and that all the younger ones did. Perseverative effects were somewhat more noticeable in the reproductions of pre-readers; and, similarly, there was some difference in the introduction of Gestalt closures. Both the overlappings of figures and the tendency to introduce syncretistic elements were of more frequent occurrence in the reproductions of the younger children than in those by the readers.

It will be obvious to the psychologist familiar with developments in the clinical use of the Bender that Hutt's categories have been modified to suit the needs of this investigation. Since the study deals with children properly classified as "normal," the psychopathological implications of the Hutt categories were not considered in making these classifications. It is of significance only to note in this investigation that the requirements of the objective situation tend to appear more obviously in the work of children who can learn to read than in the work of those who are still too young to commence the primer. Although the findings indicate that both nursery-school children and second-grade children have imperfect muscular co-ordinations, there are statistically valid differentiations revealed by the Bender. In rank-order of superiority the older children excelled in the following characteristics: orderliness of general procedure in performing the tasks; absence of syncretistic effects; freedom from overlapping one reproduction upon another; and an avoidance of an increased size in the reproductions.

Discussion

When normal second-grade children are compared with children at the nursery-school level, there are obvious differences in maturational levels revealed in Gestalt reproductions. The task-oriented responses may be partly attributed to the effects of school training in the older group. A considerable amount of work in paper-pencil situations has been done by the time a child reaches the end of the second school-year. This fact, naturally, may be of considerable moment in the imposition of the more realistic approach to Gestalt reproductions. On the other hand, the uniform testimony of experienced teachers supports the statement that a certain level of maturation must be attained before a child is able to observe these classroom requirements. In particular, the voluminous literature on the "minimal reading age" would confirm the thesis that more is involved than mere school training. The maturational factor is clearly demonstrated in this comparative study of readers vs. pre-readers. In fact, it is justifiable to say that the Bender Visual Motor Gestalt Test may be employed as a measure of a pupil's readiness to benefit by the realistic demands of the curriculum. When, in other words, a child's reproductions of the Gestalten more closely resemble those of an adult than they do those of a pre-reader, the child is likely to be regarded by experienced teachers as "normal primary-grade material" and fit to profit by the "normal" curriculum of the school.

An outstanding feature of the drawings is that those done by children in the second grade display obvious evidences of a self-critical attitude towards work of this type. Erasures are almost totally absent from the drawings of the pre-readers, but they occurred in many instances among the productions of the older children. A slower, more deliberate pace was also noted as a characteristic of the performances by the older group. In the evaluations of all the productions by both groups of children, the investigator found a marked hiatus. By the method of "blind arrangement," the work of the pre-readers could be readily differentiated from that of the "normal readers" in second-grade primers. In particular, the work of the more matured group displays a greater degree of objectivity and adaptation to the demands of the situation. Younger chil-

dren, on the other hand, did not seem to respond to the Wertheimer figures in any way suggestive of the manner of normal adults. Their reproductions are characterized by highly subjective, personal elaborations, omissions, and idiosyncracies. Both the inadequate neuro-muscular co-ordinations of the pre-readers and their unrealistic approach to task-oriented work may account for this hiatus.

Conclusions

Appraisal of the Bender Visual Motor Gestalt Test results shows that there are significant differences in at least four major determinants when five-year-old and seven-year-old "normal" children are compared. These variances may be accounted for in part by the fact that the older children have had more training in task-oriented work, with standards imposed by adults. In large part, however, they seem to be the result of important differences in maturational levels. The sensory-perceptual-motor responses of children in the second grade tend to resemble those of adults. The responses of children in the nursery school exemplify idiosyncratic examples of syncretistic behavior. In other words, reduction of autokinetic effects is the most obvious cause for the differences which may be observed in a comparative study of this type.

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THE PSYCHOMETRIC LOCALIZATION OF BRAIN LESIONS: THE DIFFERENTIAL EFFECT OF FRONTAL AND PARIETAL LESIONS ON MMPI PROFILES[1]

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Introduction

This paper is the outgrowth of a psychometric study of patients who had suffered focal brain damage and in whom the locus of the lesion had been ascertained either by surgery, *e.g.*, in cases of brain tumor, or, in gun shot wound cases, by x-ray demonstration of metallic foreign bodies in the brain. The patients studied were all male patients at the Minneapolis Veterans Administration Hospital.

The results reported here are concerned only with the multiphasic profiles of the following two sub-groups of brain-damaged patients: A) Sixteen cases with frontal lobe lesions and, B) twenty-seven cases with parietal involvement. In Group A, the lesions were, in all cases, confined to the frontal lobes, but Group B was a somewhat more mixed group in that some of these patients had lesions which extended to more than one lobe, though in all cases the common denominator of "parietal involvement" was present. With respect to laterality, there were approximately the same number of right and left hemisphere lesions.

Problem and Method

The analysis described herein was performed to test the hypothesis that there are no significant differences between the Minnesota multiphasic profiles of the patients with frontal lobe lesions and the profiles of those patients with lesions involving the parietal lobe.

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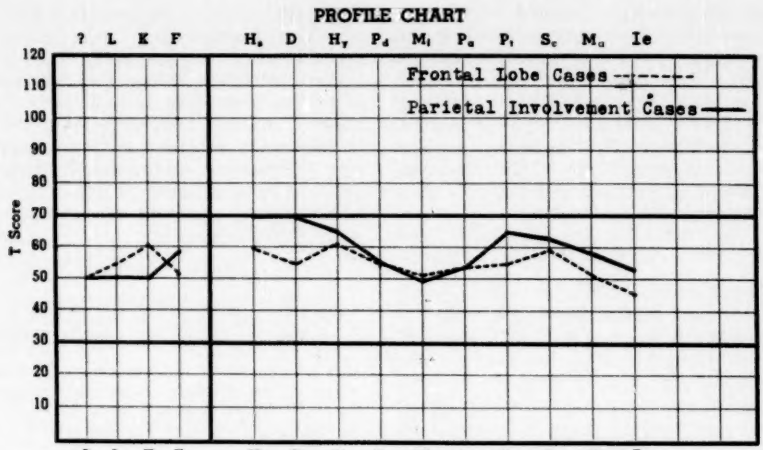


	?	L	K	F	Hs +.5K	D	Hy	Pd +.4K	Mf	Pa	Pt +1K	Sc +1K	Ma +.2K	Ie
Mean scores (frontal)	50	54	61	52	60	55	61	55	52	54	55	59	52	46
Mean scores (parietal)	50	51	51	58	70	69	65	56	49	54	65	64	58	53

FIG. 1. The mean T score profiles for 16 cases of frontal lobe brain damage and 27 cases of brain damage involving the parietal lobe.

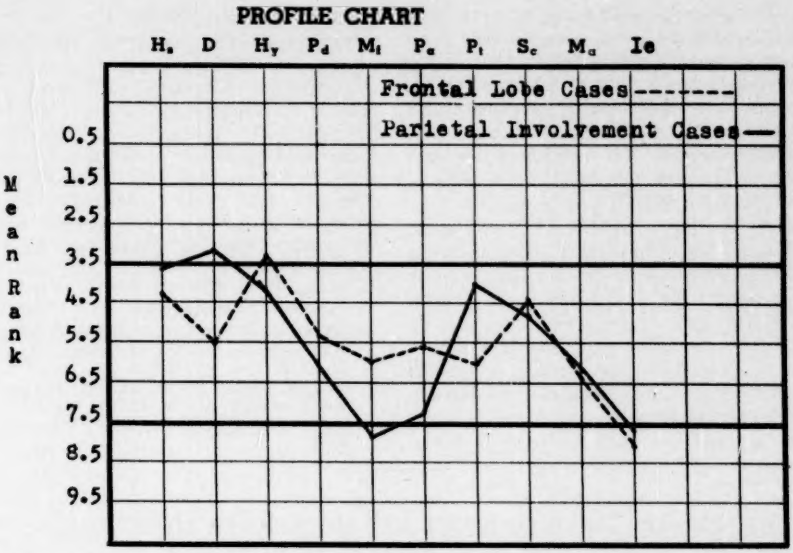
In analyzing the differences between the two groups, two methods were employed: 1) mean T scores for each group were obtained on all scales, and tests were done to determine the statistical significance of the differences which appeared; 2) profiles were "coded" by the ranking method described by Welsh[2] and the mean ranks for all scales were computed for each patient group. The results obtained by these methods are presented in Figures 1 and 2.

DISCUSSION OF RESULTS

Inspection of Fig. 1 shows that the mean T score on the K scale is significantly higher for the group of patients with frontal lobe lesions. However, on the F, D, Pt, and Ie, scales the parietal involvement group had significantly higher mean T scores.

With respect to variability, the following differences were noted: on the L scale, the frontal lobe group is significantly more variable; on Hs, Hy, and Ie, the parietal involvement group is the more variable. A possible reason for the greater variability of the parietal involvement group on the clinical scales is that this group is a more variable group with regard to location of brain damage. For the frontal lobe cases all had lesions exclusively in the frontal lobe, while the parietal lobe cases showed overlapping of lesions into other lobes and were thus not as pure a group neurologically.

An interesting fact is that the differences in mean T scores which have been referred to above, suggest that the patients with parietal involvement have more severely disturbed personalities than do the patients with frontal lobe lesions. In addition to and complementing this difference in severity, there is also a difference between the two groups in *type* of personality involvement. The mean profile for cases of frontal lobe injury exhibited a V-shaped neurotic triad (*i.e.*, the Depression score is below those of Hypochondriasis and Hysteria). There is also a secondary elevation on the Schizophrenia scale. The "composite"



Mean ranks (frontal)	4.3	5.6	3.4	5.4	5.9	5.7	6	4.4	6.3	8
Mean ranks (parietal)	3.6	3.2	4.2	6.2	7.8	7.3	4	4.8	6.3	7.6

FIG. 2. The mean rank profiles for 16 cases of frontal lobe brain damage and 27 cases of brain damage with parietal lobe involvement, illustrating relative scale relationships in contrast to the absolute T score relationships shown in Fig. 1.

profile code of the frontal lobe cases (using Welsh's method of coding, ref. cit.) is: 3184265790. From this profile, one might conjecture the mean personality type of the frontal group to be capable of description somewhat as follows: accepting, non-irritable, not anxious, affable, easy going, and possessed of a relatively low general level of aspiration. This description does, in fact, fit the most common clinical picture observed among the individual patients in this group.

The mean profile for the patient group with parietal lobe involvement is quite different from the profile just described. Here, the neurotic triad scores present an *inverted* V-shape (with D high) and the secondary peak is on the Psychasthenia scale rather than on the Schizophrenia scale as is the case in the frontal lobe group. When we consider individual cases, a higher proportion of markedly abnormal profiles are found among the parietal involvement group. Profiles capable of being interpreted as normal are far more common among patients with frontal lobe damage than among patients with parietal involvement.

The Welsh profile code for the parietal group is: 2713890465. This profile might be interpreted as reflecting an "anxiety neurosis" type of personality: *i.e.*, rigid, worrying, pessimistic, hyper-active, precise and detail-bound. As a group, the patients making up this parietal involvement group appear more sensitive to anxiety-producing environmental situations than are the frontal lobe patients or than are normal individuals without brain damage. This sensitivity and the general anxiety-prone personality type are clinically observable in many individual cases in the parietal involvement group. This type of personality is rare, if not nonexistent, in the frontal lobe group of patients.

The differences observed between these two localization groups suggest the presence of some cerebral localization of the reaction patterns which influence the behavior of an individual in response to his environment and which are reflected on MMPI records. Of interest in this connection, for the purposes of comparison, are the mean MMPI profiles published by Hovey[1]. His composite profile for cases of dissociative-conversion reaction has a marked resemblance to that presented in this paper (Fig. 2) for cases with frontal lobe damage. Hovey's composite profile for cases of anxiety neurosis, on the other hand, resembles the profile presented herein (Fig. 2) for cases of brain damage with parietal involvement.

SUMMARY

The results of a study of MMPI profiles of patients suffering focal brain damage are presented. Characteristic T score and pattern differences are shown between patients with frontal lobe lesions and those with parietal involvement. The former group is shown to have both psychometric and clinical indications characteristic of the hysteroid type of reaction pattern; the latter group most closely resembles patients suffering with anxiety neurosis.

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EVALUATING AND SCORING THE MINNESOTA MULTIPHASIC "CANNOT SAY" ITEMS

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INTRODUCTION

A primary place among personality inventories in clinical diagnosis and vocational counseling is being attained by the Minnesota Multiphasic Personality Inventory. Its 550 statements constitute most of the more important questions asked a patient in an initial psychiatric examination. Apart from an analysis of the profile, the clinician or counselor can profitably inspect responses to individual items, using the Inventory somewhat like a questionnaire.

The MMPI is completed by the patient answering each of the statements as "true or mostly true," or "false or not usually true," as applied to him[1]. He relegates certain cards to the Inventory's *Cannot Say* division when the item does not apply to him, or if it is something he doesn't know about. However, the client is urged to resolve every test item as either True or False.

The total number of cards put in the *Cannot Say* group make up the Question or ? score. It is a validating score, and its size ". . . . affects the significance of the other scores. Large Question scores invalidate all others. A 'borderline' Question score probably means that the subject's actual score, if he had not used the *Cannot Say* category at all, would deviate farther from the average than his observed score indicates"[2].

T tables have been set up for the raw scores on each scale of the Inventory. A score of 1 represents one statement or card that appears in the counting. A Question score of 30 or less counts as a T value of 50, or "normal." Still considered within the normal range, rating a T of 69, is a raw score of 100, representing 100 cards placed in the Cannot Say group. Hathaway and McKinley consider that "Question scores above the borderline T score of 70 should be taken

* The opinions expressed are those of the writer. They do not necessarily reflect the viewpoint of the Veterans' Administration.

as a sign of invalidity, and some allowance should be made in the range of 60-70"[2].

The authors of the MMPI stress the importance of making sure that each single card is in the box (individual test), when the examinee is through working on the Inventory. This diligence is entirely warranted. And yet, the client can eliminate many cards from scoring consideration simply by isolating them in the ? section. This article suggests that certain ? cards be included in the scoring of the clinical as well as the validating scales, and presents a rationale and trial procedure for doing so.

Rationale of the ? Category

We see a large number of Inventory items omitted from scoring by the "either-or" logic of the Cannot Say device. The ? unit appears to have its reason for being in the following assumptions: If the patient cannot, or will not (same thing?), decide whether a particular statement is either true or false, or if it does not pertain to him, then it is neither true nor false, and should be left out of the scoring scheme. It is proposed, as an hypothesis, that certain ? items be regarded as having *some* truth in them. This reasoning is used: That which is not entirely true, or mostly or usually true, and yet is not entirely false, is *partially* true.

Under what circumstances does an examinee place cards in the Cannot Say compartment? Among the several reasons that may be conjectured, these seem to merit special attention:

1. The items do "not apply to him," or "he doesn't know" about them. Two examples follow:
   a. Item B-50—*My people treat me more like a child than a grown-up.* To a mature man who had left the parental home years ago, and had not been in close contact with his relatives, such a statement would not apply. It may have been answered in the affirmative if it related to an earlier period of his life. But testees are supposed to answer the statement in the tense in which it is presented.
   b. Item J-26—*I liked "Alice in Wonderland" by Lewis Carroll.* This statement may possibly be one that the client doesn't know about, or does not remember as to whether he liked it or not.
2. Patients unable at times to be decisive even in clear-cut issues are likely to fill up the ? category with their pathological irresoluteness. The MMPI Manual recognizes that "In its own right the Question score is an indicator of personality factors, but no specific clinical material on it has been analyzed. High scores have often been observed to occur in psychasthenia and retarded depression patients."
3. The examinee falsifies a card by declaring that he cannot say whether it is true or false, when he really can or should be able to.

At this point, a brief discussion of lying on the MMPI is in order. The L(Lie) scale provides a measure of the extent to which the client tries to falsify by presenting a socially approved picture of himself. In other words, his tendency to put particular statements that are true for him into the False group, and vice versa. But perhaps it is just as much a lie when he places a card, that represents a true situation for him, in the ? section. As none of the latter cards are ever replaced into the Inventory's scoring, the L scale to that extent is limited in its measurement.

The three reasons suggested above, of course, do not exhaust the listing of possible motivating factors behind the examinee's rejection of MMPI items. But they are probably the main ones.

Scoring Selected ? Items

It would seem, therefore, that the only ? cards that should really remain unscored are those first discussed above—those that do not apply to the patient or that he doesn't know about. The others, it is postulated, should be returned to the scoring scheme. (Hereafter in this article, "—?" will refer to a ? card that is not to be returned for scoring, and "+?" to the ? statement that is.)

Just which statements placed in the Cannot Say division should be given a — ? value? There is a number of such items relative to family relations, for instance, which would not pertain to a mature adult who had severed family ties years before. Item B-50, mentioned earlier, is a sample. Generally, most of the items in the B 47-55 group, and those numbered C 1-16, might be considered in this respect.

Once the exact cards that qualify for the —? group are selected, they can be inconspicuously marked. A red or black dot on the back of the card may serve to identify it. In using the individual test (box of cards), experimental scoring could be done thus:

1. Take out all the cards that have been put in the Cannot Say department. Discard all those with the special mark on the back, the —? cards.
2. For each remaining +? card, enter a ? mark on each line of the record sheet as indicated by the card's number. No entries are made for the —? statements discarded.
3. When applying the template for the particular Inventory scale to be scored, count the ? marks that correspond to either X's or 0's on the key. But while valuing each of the regular scored items as 1 point, experimentally rate each scored ? mark as ½ point. The total of the ones and the one-halves would then be the raw score for the specific scale.

The reason for assigning a trial value of ½ to the ? mark scored is this: Those True and False cards which qualify for scoring have each a value of 1. The +? card is considered as falling somewhere between this point and zero, and halfway between is a logical trial point.

In the usual scoring procedure, then, a ? mark is entered on the record sheet for each card in the Cannot Say section, without separating the cards. When a question mark is aligned with an X on the template, it is not scored. When contingent to a 0 on the key, according to Krise[3] and others, it is not counted either because the cell is not actually blank. In the proposed plan, on the other hand, the Question cards are sorted into —? and +? groups. The —? items are not entered on the record sheet. Only the +? cards are, with the usual ? mark as the entry symbol. While in the present MMPI scoring system no ? mark is ever counted, each such mark in the experimental plan is given a ½ value when eligible for scoring on the individual scale.

All of the cards in the individual test have either the lower right or lower left corners cut off. Only those cards put in the True section with the right corner cut off, and those in the False group with the left corner severed, are actually counted. They are statements that are in the "abnormal" direction, as distinguished from the others, discarded and unscored, which are in the "normal" or expected direction. The following example illustrates the principle which places +? cards from the Cannot Say division into the counting scheme, irrespective of which corners are cut off:

Item A-46—*I have few or no pains* (left corner missing). The statement is within the experience or knowledge of all persons, and every examinee is expected to resolve it as being true or false. When the card is placed in the True compartment, it does not enter into the final counting because it denotes a normal response. If sorted in the False unit, it indicates an abnormal condition and scores 1 point. But if isolated in the Cannot Say partition, it is omitted from scoring altogether *just as if* it had been regarded as true. Since it is a statement that every client "knows about," it assumes +? significance when thus sorted, and is scored, but at ½ value. This principle of the +? item appears to hold true for the clinical, validating and corrective scales alike.

So far, discussion has centered on the individual MMPI test, the box of cards. The plan advocated here, for scoring +? items, is equally applicable to the group form. On the latter, the patient reads each statement from a booklet, and marks appropriate spaces on an answer sheet for True or False responses. When he "cannot say" on an item, he enters no mark on the answer sheet for that state-

ment. Experimental scoring of the group test can be performed in this way:

1. Identify each statement left blank on the answer sheet, having —? value, by a dot or other sign next to the item number. (The statements not to be replaced into the scoring scheme from the Cannot Say section are now segregated.)
2. With a colored pencil, fill in the blank spaces under *both* the True and False columns of the remaining blank statements. (This assures scoring of the +? statements.)
3. As each template is applied in turn, the colored markings that appear through the holes are each valued at ½ point, and added to the total of the penciled markings (1 point value) to make up the specific scale's raw score.

Discussion

The Cannot Say or ? category in personality inventories plays an important role in psychometry. For one thing, it does serve as a container for those items which the client considers do not pertain to him. But in addition, it provides a protective cage, so to speak, where the examinee can retire at will when he feels threatened. Unless a personality test includes such a "protective" device, the antagonism of a client toward the rigidity of the True-or-False system of choice may result in his refusal to cooperate in the examination. To a more compliant subject, the situation may prove most distressful, and continuing working on the Inventory to the completion of the 550 items could well prove harmful to him. Particularly is this true of neuropsychiatric patients.

It is apparent that the —? cards placed by one subject in the Cannot Say section may fully pertain to him, and those similarly rejected by another may not. But it is evident that many complications would result were each client to be evaluated accordingly on an individual basis. In order to continue the objectivity and relative simplicity of the MMPI scoring method, it would be generally feasible to regard all —? cards set aside in the Cannot Say category to be pertinent to the examinee. In other words, a card with the special —? marking is just an ordinary card as long as it is sorted as true or false. Once it is put in the Cannot Say compartment, however, it assumes a —? value, *i.e.*, considered as justifiably placed there by the patient because it does not pertain to him or he doesn't know about it.

To indiscriminately consider the Question score in relation to personality factors would therefore be an error. Suppose a correlation is actually found between the size of the Question score and personality, perhaps psychasthenia or depression. Patient A puts 60 cards in the Cannot Say division. Half of those are warranted, as they have —? value. Patient B also defers 60 items, but none are classified as —?. Patient A's Question score could hardly be regarded in the same light as that of B's.

This brings up another aspect of the problem. After all the items are sorted by the patient, the authors of the MMPI suggest that the Cannot Say section be inspected, and "If more than about one tenth of the cards have been put in the *Cannot Say* category, the subject should be encouraged to place more in the other categories." But what if most, or even some, of the items are —? cards and are therefore validly isolated? Our "encouragement" leads the examinee to give spurious value to those statements by his replacing them into the True and False groups. The discriminate client may be puzzled to first be told that he place cards that don't pertain to him in the Cannot Say partition, and when he really does so with the —? cards, is urged to reverse himself and declare them and his other Question cards as true or false. This wholesale clearance of the Question section has been accepted as a virtue, for it has brought that many more items into the scoring fold. From the frame of reference of this paper, certain (—?) items may validly be segregated in the Cannot Say section.

From this discussion, it would appear that the initial instructions to the examinee would be given different emphasis: He would be instructed that certain statements may not apply to him *at this time*,

and others he may not know about. These he may place in the Cannot Say division. But the remainder more or less do apply to all persons, and those he should try to place in the True or False sections.

When the client is through with the sorting, no matter how many cards are segregated as Cannot Say, he is not requested to resolve them. The examiner simply removes the —? items from this stack, and records the remainder as described. The size of the original Question score would then have no bearing on the validity of the test as a whole.

It is believed that this practice, when standardized, will add to the delineating value of the MMPI's validating, corrective and clinical scales.

Summary

1. As many as 100 MMPI statements may be eliminated from scoring when placed in the Cannot Say category by the client. This tends to constrict the ratings of the various scales.

2. The circumstances under which cards are put in the Cannot Say section are discussed briefly.

3. Certain Inventory items may validly be sorted in the Cannot Say division, and when thus classified, the examinee should not be urged to resolve them as true or false. These items are given "—?" significance.

4. All other Cannot Say cards should be returned to the scoring system because they more or less apply to all persons. Such statements are assigned "+?" value.

5. It is pointed out that a card isolated in the Cannot Say group, which can be answered true or false (all +? cards), is actually a falsification. This particularly limits the function of the Lie score.

6. There remains the final selection of statements having —? significance, and experimental investigation to determine whether the return of the +? cards to the scoring scheme will improve the diagnostic value of the MMPI.

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THE USE OF Z SCORES IN EQUATING THE WECHSLER-BELLEVUE SUBTESTS*

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Introduction

The importance of evaluating the subtests of an intelligence examination in addition to the mere reporting of an I.Q. is recognized by most clinicians. The Wechsler-Bellevue Intelligence Scale attempts to equate the scoring for all subtests by means of weighted scores in order that a direct comparison of the subject's performance on the various subtests can be made. Thus, Wechsler[4, p. 148] states:

"The second clinically useful feature of the Bellevue Scale is that the different abilities tested by it may be compared at all levels of functioning. This is achieved through the fact that the same type of material is used throughout the scale, and because the individual subtests of the scale have been equated."

* The writer acknowledges with gratitude the suggestions offered by the following at various stages in the preparation of this paper: Drs. George Copple, Kate L. Kogan, Saul Rosenzweig, Carroll Whitmer, Erma T. Wheeler, and Mr. M. V. Taylor, Jr. Most of this paper was written while the author was a member of the hospital staff of the Mayview State Hospital, Mayview, Pa.

Unfortunately, this method fails to take into consideration the variations among the means of the subtests in the standardization group at various age levels and assumes that a given weighted score represents the same level of ability on every subtest. This fallacy is exceptionally clear in the extreme age groups where there are large differences in the means and standard deviations of the tests. In the 55-59 age group the mean weighted score for Information is 9.5 while it is only 5.9 for the Digit Symbol [4, p. 222]. Should a subject obtain approximately the above weighted scores on the subtests, the comparison might falsely indicate an impairment on Digit Symbol. In addition, the difference in standard deviation, amounting to 1.33 between Arithmetic and Digit Symbol is also not negligible. Likewise, at the 17-19 age level the mean for Digit Symbol is 11.1, while that for Arithmetic is 8.5. These inadequacies of the Wechsler-Bellevue provide grounds to challenge the accuracy of Wechsler's "diagnostic signs" and Rapaport's[3] elaborate diagnostic studies, together with the many others that have appeared in the literature.

Foster[2] has re-weighted the means of the subtest scores as a solution to this question. In his discussion he indicates that individuals doing research with the present scoring might overcome this defect by utilizing groups of similar age in their studies. However, it should be pointed out that this method would permit comparisons of a given subtest among these subjects, but it would not allow for comparisons among subtests. Since the technique of "pattern analysis" involves the latter, a new method for analyzing the test results is unquestionably desirable.

The Z Score Method

As a refined technique of equating the tests, we have constructed a series of tables (available from the author) with *standard scores* (Z) in place of the weighted scores obtained on the subtests. The calculations were made by algebraically subtracting a given weighted score from the mean of that subtest at the particular age level. This amount was then divided by the standard deviation. This method makes it possible to compare all subtests with each other at any age level.

The tables are arranged so that the clinician can ascertain, at a glance, the corresponding Z score for a weighted score ranging from 0 to 18. Separate tables are provided for all age groups, 17 to 59. The data used in these calculations were obtained from the appendix of Wechsler's manual. Unfortunately the mean and standard deviation for the Vocabulary subtest are not listed. In a personal communication with Wechsler, the writer was informed that "the original data are not available at present." The tables therefore do not include Vocabulary, but the Z scores can easily be added when "norms" become available. Since all data presented here are from Form I, additional tables would be required for Form II.

Through the use of the method described above, diagnostic "signs" and "ratios" could assume a greater validity. A re-examination of past research may produce changes in present diagnostic "formulae."

A psychograph is provided on the revised record form for use with the Wechsler-Bellevue[5], but Wechsler cautions about its "unreliability." In using Z scores a psychograph can be drawn by plotting the standard scores as shown in the sample reproduced and described below in fig. 1. The horizontal line at *zero* is the "norm" or "assumed mean" for all subtests. Standard score deviations from the mean of an individual's Z scores are marked off above or below the line to represent scores in the same relation to the subtest "norm." This method affords a comparison of the individual's scores with others of his own age group, and graphically presents what is really desired in an intelligence test.

Wechsler[4, p. 149] states that within the range of total scores from 80 to 110 a mean deviation of more than two points is significant, since, "This represents a deviation of approximately $\pm$ 1 S.D. from the mean of 95 at ages 20 to 35." Significant differences for scores not included in these limits are obtained by dividing the mean subtest score by four. The writer believes that when the analysis of scores is used for individual diagnostic purposes, it is preferable to spend the added time required to calculate a standard deviation.

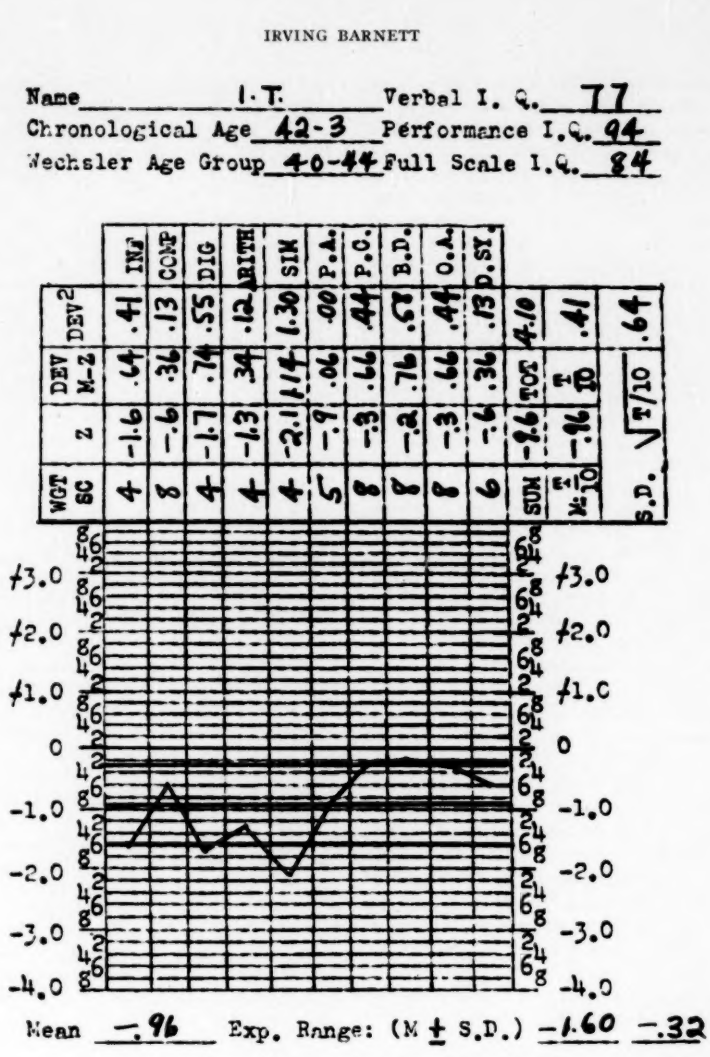

FIG. 1. Illustrative Wechsler-Bellevue Psychograph.

Accordingly, to simplify the procedure, a form consisting of columns and headings indicating each mathematical step involved in the computation is combined with the psychograph.

In order to fill in the data it is necessary to keep the graph sideways, as this makes possible a compact use of the form. The usual statistical steps follow: (a) The weighted scores are first inserted for each of the subtests. (b) These are then converted into Z scores by means of the tables provided, and also entered and joined on the graph itself. (c) The algebraic sum of the Z column is then calculated and divided by ten to obtain the mean. (d) The standard deviation is derived as follows: Find the deviation of the

Z scores from the mean; square each deviation[1]; calculate the total of the squares; divide by ten; obtain the square root. (e) The expected range for the individual's scores is found by adding the standard deviation to the mean for the upper range and subtracting it from the mean for the lower range. These points are entered on the graph by marking off horizontal lines in the appropriate positions.

A glance at the graph reveals that although four tests have a weighted score of *four*, their Z scores indicate a scatter. The four subtests with weighted scores of *eight* also vary, but to a lesser degree. However, Comprehension and Digit Symbol are now on the same level in spite of the two points of difference between their weighted scores.

Diagnostic Implications

The comparison of profiles in table 1, constructed through the use of weighted and Z scores, illustrates some of the defects which are inherent in the present method for scatter analysis.

Table 1. *Illustrative analysis comparing weighted scores and Z scores for a sample case.* C.A. 56-0 Verbal I.Q. 89 Perf. I.Q. 91 Full scale I.Q. 86 Diagnosis: Schizophrenia

Subtests	*Wgted. Scores*	*Z scores*
Inf.	7	— .6
Comp.	6	—1.0
Digits	7	— .2
Arith.	6	— .2
Simil.	4	—1.2
P.A.	4	— .9
P.C.	3	—1.3
B.D.	5	— .5
O.A.	5	—1.0
D.Sym.	4	— .7
Mean	5.1	— .76
	S.D.	.37

Organic Involvement. We might examine this profile for the possible presence of organic involvement:

According to Allen[1], Information, Comprehension, and Vocabulary are most resistant in a brain diseased person, and he states that either of these may be used as a point of orientation for scatter analysis. If weighted scores are applied here, both Information and Digit Span appear together at about two weighted points above the mean, which Wechsler considers the "normal range." All other subtests, save Digit Span, fall below Information which may be considered as a "base" in this analysis. When Z scores are used, however, Information drops to a position about midway between the mean and ± 1 standard deviation. It is now below the Z scores of Arithmetic and Block Design, in addition to Digit Span, and only slightly above Digit Symbol. Comprehension, which appears above the mean on Wechsler's profile has also now fallen considerably.

The diagnostic implications of the above are apparent. The vulnerability of Information and Comprehension in the new profile is on less stable ground. Allen's[1] prediction that Digit Symbol and Block Design will show impairments in organic brain disease is confirmed according to the weighted scores, yet contradicted when the same subtests are represented by Z scores. These same items are now above the mean, or more important, not significantly below Information or Comprehension. Object Assembly, which is also classed as a "susceptible" test item, is at the mean on the older profile but much lower on the revised form.

Schizophrenia. When the same profile is analyzed with respect to schizophrenic indicators as developed by Wechsler[4, p. 150], the Z scores again help to highlight positive factors, as shown below:

"Sum of Pict. Arrangement plus Comprehension should be less than Information and Block Design." When the corresponding values are calculated on the basis of Wechsler's weighted scores, the formula becomes 4 + 6 (10) as against 7 + 5 (12), or a ratio of *.83*. The alternative figures are −.9 + −1.0 (−1.9) over −.6 + −.5 (−1.1) and a .58 ratio.[2] Wechsler's expectation that Picture Arrangement and Comprehension would be *less* is more strongly substantiated.

"Object Assembly much below Block Design." Both weighted scores are *5* but

1. Tables of squares are provided in most statistics books and can simplify the work involved here.

2. Since the better scores are indicated by lower minus values, the ratio for Z scores is calculated by inverting the sums.

the Z equivalents become —1.0 and —.5 respectively, and indicate a positive sign not apparent otherwise.

"Very low Similarities with high Vocab. and Inf. definitely pathognomic." Since Z for Vocabulary is not available, we are limited to a comparison of Similarities with Information. Scores here are 4/7 with a .57 ratio, while Z values are —1.2/—.6 or a ratio of .50.[2] Here again the latter is more in the positive direction.

Discussion

It is amazing that the Wechsler-Bellevue has achieved such wide popularity and has been subjected to such intensive study for its diagnostic implications in spite of the limited population on which it was standardized. Furthermore, the so-called "weighted scores" have been utilized for clinical work and research in a manner that disregarded their statistical nature.

The method of equating subtest scores described here does not solve the basic need for broader norms, but it attempts to solve one of its failures. It is recognized that a refinement such as this may not be justified in view of the above; yet if it is to be utilized clinically in its present form, we may as well make the best of it. When a better standardization is completed, the techniques described in this paper should become still more useful.

Summary

Although the Wechsler-Bellevue Intelligence Scale weights its subtest scores to facilitate comparing an individual's performances, actually the scores are not equated with statistical accuracy. A method of correcting this fallacy through the use of Z scores is suggested and a psychograph using this revision is described. Since previous research on diagnostic aspects of the test overlooked these statistical errors, a re-examination of the accumulated data is proposed.

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WECHSLER-BELLEVUE SUB-TEST PATTERNS: A NOTE OF CAUTION

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Introduction

At the present time there is considerable interest in the use of the sub-test patterns on the Wechsler-Bellevue Scale of Adult Intelligence for purposes of differentiating the normal, feebleminded and mentally ill. Attempts at such analysis are not new, but have also been tried with the Stanford-Binet[2, 6]. Certain previous studies with the Wechsler-Bellevue have attempted to differentiate specific psychiatric groups but with little success[1, 3, 4]. Wechsler has published patterns for feebleminded, psychopathic, organic brain damage, schizophrenia and the like. Rapaport's study attempts to differentiate among the different types of psychoses and psychoneuroses. His sampling of subjects is, however, not as adequate as Wechsler's as it is very highly selected. Wechsler's subjects are selected in terms of locale. This criticism appears to be more important with regard to some sub-tests than others,

* This study was completed and written while the author was at the University of Hawaii.

more important for some cultural groups than others. According to Wechsler[8], feebleminded and psychopaths should have Performance Scores higher than Verbal, all other psychiatric syndromes should have Verbal Scores higher than Performance Scores. Rapaport[5] does not report such gross differences, but his diagnostic charts indicate that on the whole Verbal Scores would be higher than Performance Scores.

Method

This investigation is a survey of 155 unselected referrals to the Psychological and Psychopathic Clinic of the University of Hawaii to determine to what extent they would conform to the diagnostic patterns in use. These cases include both males and females. The age range is from about 14 to 50 years of age. The sources of referral are mainly from the Juvenile Court and Adult Probation Department. The original tabulations differentiated among the various national groups, but it was found that there were too few in each group to make such analysis meaningful. However, it might be interesting to mention the national make up of the group; there were Hawaiian, Chinese, Japanese, Korean, Caucasian, Portuguese, Filipino, Puerto-Rican, Hawaiian-Oriental, Hawaiian-Caucasian and miscellaneous combinations. The cases were divided into two groups, those whose Verbal Scores were higher than the Performances Scores and vice versa. Tabulation was done according to a five point scale of differences in the two scores. The number of mental defectives falling in each grouping was tabulated. There were too few cases in each group to make percentages statistically permissible. There were only sixteen cases on which psychiatric evaluation was available and these included several different types of disorders so only a brief qualitative statement will be made with regard to them.

Results

The statistical breakdown of those cases whose Verbal Score was higher than the Performance Score indicate there are thirty-four or 22% such cases. For those whose Performance Score was higher than the Verbal Score there are a hundred and twenty-one or 78%. There are twenty mental defectives in the whole group of a hundred and fifty-five cases, 50% having Verbal Scores higher than Performance Scores and 50% Performance Scores higher than the Verbal. Thus of the first group mentioned, 29% are mentally defective while of the second only 8% are defective. Obviously, these per cent values are the opposite of what one has been led to expect. As a matter of fact, only 15% of the mentally defective cases show the expected deviation on the two scales, that is only 15% show a difference of eleven points or more in favor of the Performance Scores.

The results concerning psychiatric syndromes are only suggestive because there are only sixteen cases covering varied diagnoses as previously mentioned. These diagnoses were psychoneurosis, schizophrenia, psychopathic personality, and organic brain damage. According to Wechsler, psychopathic personalities should obtain higher Performance than Verbal Scores, the other psychiatric conditions should have higher Verbal Scores. It is sufficient to note that these sixteen cases do not conform to established test patterns, but are scattered throughout both distributions, *i.e.*, Verbal Scores higher than Performance and Performance Scores higher than Verbal. The cases diagnosed as organic brain damage do have higher Verbal than Performance Scores, but none shows a significant deviation.

It seems apparent from the results of this investigation that the published subtest patterns of the Wechsler-Bellevue Scale are not applicable to the clientele of the Psychological Clinic of the Territory of Hawaii. This is true for the patterns of both Wechsler[8] and Rapaport[5]. Rapaport bases his patterns on deviations from vocabulary scores. For the majority of the cases in this study the vocabulary score is frequently the lowest of the verbal scores, and even if one substitutes the total Verbal Score, it is obvious that such a procedure will have little value with our population.

These results are particularly noteworthy with regard to the mentally defec-

tive, and it is important to the interpretation of these facts that the psychologists of the Psychological Clinic are extremely conservative in respect to diagnosing individuals as defective. Thus, even if an individual scores low on testing but is making a satisfactory social adjustment, *i.e.*, if there are no complaints from family, neighbors, social or law enforcing agencies, such a person is not deemed to be feebleminded or subject to institutionalization.

The results suggest the same inapplicability with regard to psychiatric syndromes. Perhaps the outstanding characteristic of our population as compared with similar populations is bilingualism, even if the second language is only "pidgin." It is believed, then, that considerable caution must be exercised in applying these test patterns to the population sampled by these data, and perhaps, to any bilingual population or individuals.

Summary

1. This investigation is a survey of 155 cases in the Territory of Hawaii to determine the validity of the Wechsler-Bellevue diagnostic patterns for various clinical entities such as feeblemindedness and psychiatric syndromes.

2. The results indicate that these patterns are not valid for the population studied. The results are most significant as regards feeblemindedness. More than three-fourths of the cases cited have Performance Scores higher than Verbal and only 15% of the feebleminded show the expected deviation in scores. There is a suggestion that feebleminded are about equally inferior in verbal and performance tests. A strong tendency of similar inapplicability is evident for psychiatric diagnoses.

3. Therefore caution must be exercised in depending on published patterns for any type of diagnosis. This does not reflect on the usefulness of the Wechsler-Bellevue Scale as a test of general intelligence.

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THE EFFECT OF LATERALITY LOCALIZATION OF BRAIN DAMAGE ON WECHSLER-BELLEVUE INDICES OF DETERIORATION[1]

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Introduction

In the course of routine examination of patients on the neurology wards of the Minneapolis Veterans Hospital it was observed that the indices of deterioration used in connection with the Wechsler-Bellevue Intelligence Scale[7] often completely failed to identify definitely established cases of focal brain damage. (The term focal brain damage, as used here indicates damage of a relatively confined cerebral area as opposed to generalized brain damage, defined as damage involving most or all of the cerebrum. Generalized brain damage is secondary to conditions such as toxic or infectious encephalitis, whereas focal brain damage is commonly secondary to brain tumor, gunshot wound or similar trauma.) This condition appeared to be related to the laterality localization of the focal injury to the cerebrum. The present paper reports an investigation of this relationship together with an investigation of the general efficacy of two Wechsler-Bellevue indices of deterioration among cases of definitely established brain damage.

The two indices of deterioration examined in this study are the Wechsler-Bellevue Deterioration Quotient and the Reynell Index. The Deterioration Quotient (hereafter DQ) is in wide general use and is adequately defined by Wechsler[6, pp. 65-67]. Therefore, no further description will be given here.

The Reynell Index (hereafter RI) was described in a publication by Reynell[4] in 1944. It makes use of the Verbal subtests only. RI is calculated by deriving Verbal IQ's from each of two groups of three Verbal subtests. The deterioration-sensitive group is composed of the Digit Span, Arithmetic, and Similarities subtests. The remaining three Verbal subtests make up the group considered relatively insensitive to deterioration. The weighted score sums for these two groups are multiplied by 5/3, since the Wechsler tables of Verbal IQ's are based on sums of five subtest weighted scores. The difference between the two IQ's thus derived is the Reynell Index, the deterioration-sensitive IQ being subtracted from the non-sensitive IQ. A difference of plus ten or more is considered indicative of significant loss. The RI was developed and evaluated in England in terms of predicting success in rehabilitating patients who suffered head injuries during the last war.

The subjects used in this study were fifty-five male veterans of World Wars I and II. All were seen as either inpatients or outpatients at the Minneapolis Veterans Hospital. The age of the patients ranged from nineteen to sixty-one years; the mean age was 32.47 years and the median age 26.77 years. Comparison with population statistics indicated that the education of the group was superior to that of the general population; the median years of school completed for the forty patients aged twenty-five years and over was 11.61; the comparable median for this age group in the general population is 8.7 years[5, pp. 125-126]. There were no statistically significant differences between the occupational classification of the group and that for the general population[5, p. 190].

All patients used in the study had suffered focal cerebral brain damage confined to one cerebral hemisphere, as far as could be determined. All cases were either surgically substantiated as brain damaged or, in the case of a few gunshot wounds, established by X-ray evidence of retained metallic foreign bodies within

1. The material contained in this paper is abstracted from a Ph.D. thesis presented to the University of Minnesota Department of Psychology. The paper is published with the permission of the Chief Medical Director, Department of Medicine and Surgery, Veterans Administration who assumes no responsibility for the opinions expressed nor the conclusions drawn by the author.

the brain substance. The presence of definite injury to the cerebrum was established beyond any reasonable doubt in every patient included in the study in consultation with members of the neurological and neurosurgical staff. However, the criterion of unilaterality is probably relatively poor. Such conditions as unreported previous trauma, possible contrecoup injury and even the possibility of surgical misdiagnosis may have contributed to the fallibility of proper laterality localization. Studies employing perfect localization criteria will probably have to await the collection of cases with available post-mortem examinations.

The total sample of fifty-five cases was also treated in two subgroups. A subgroup of thirty cases of non-dominant or recessive cerebral hemisphere injury and a subgroup of twenty-five cases of dominant hemisphere injury were employed. As used in this study the terms dominant and non-dominant, or recessive, refer only to the cerebral hemisphere contralateral to the preferred hand and ipsilateral to the preferred hand, respectively. Hand preference was determined by observation and interview for each case.

Only one statistically significant difference appeared between these two subgroups with regard to the statistics reported above for the total sample. The non-dominant hemisphere subgroup (hereafter the NDH subgroup) was significantly older ($P < .01$) than the dominant hemisphere subgroup (hereafter the DH subgroup). Probably the best hypothesis to account for this difference is that a dominant hemisphere injury is more liable to result in permanent aphasias in the older age group where some degree of normal senescent loss has already occurred. Also younger men tend to be more responsive to retraining after developing severe aphasic disabilities. Either of these conditions would operate as a selective factor to produce the observed age difference, since all patients included in the sample were capable of communication and cooperation. If the older dominant hemisphere patients were relatively more disabled and, consequently, less testable, their inclusion in the sample would be relatively rare.

Procedure

Form II of the Wechsler-Bellevue Intelligence Scale[7] was administered to the patients by psychologists experienced in the use of the test. After the cases were collected all tests were checked for accuracy of addition and, in the case of the more subjectively scored subtests, for consistency of scoring.

The Wechsler DQ was calculated by the usual method for each case. The medians, ranges, means and standard deviations of the DQ distributions for the total sample, the NDH subgroup and the DH subgroup were obtained. Wechsler [6, pp. 65-67] states that a "per cent" loss of ten (DQ of 90) indicates possible deterioration, while a "per cent" loss of twenty (DQ of 80) indicates definite deterioration. Accordingly, the percentages of cases falling below these two values in the total sample and its two subgroups were calculated.

The same statistics were obtained for the distributions of the Reynell Index for these groups. The percentages of cases falling below the cutting score of plus ten IQ points difference suggested by Reynell[4] were calculated.

Pearson correlation coefficients between the two indices and between each index and age within the total sample and the two subgroups were calculated.

Results and Discussion

Tables 1 and 2 give the results of the application of the Wechsler DQ in the total sample and the NDH and DH subgroups. In terms of the means and medians shown in Table 1, all three distributions tend to be negatively skewed, the tendency being stronger in the DH subgroup. Both the mean and standard deviation of the DH subgroup are lower than the comparable statistics for the NDH subgroup. These differences, however, do not reach statistical significance. Apparently, the possibility that these sample differences are due to an association with the age variable, which showed similar trends, must be rejected: the Pearson r of the DQ with age in the total sample is —.12, in the DH subgroup —.11, and in the NDH subgroup —.06.

TABLE 1. *Results of the application of the Wechsler DQ to the total sample of focally brain damaged patients*

Sample	*N*	*Median*	*Range*	*Mean*	*S. D.*
Total sample	55	84.25	36-122	83.20	16.30
NDH subgroup	30	86.50	36-122	85.53	16.46
DH subgroup	25	82.00	38-95	80.40	15.64

Table 2 gives the results achieved by applying the two cutting scores suggested by Wechsler to the present sample of authenticated cases of focal brain damage. Nearly a third of the total sample show less than the ten "per cent" loss indicative of "possible" deterioration, while nearly two thirds of the cases fall below the cutting score of twenty designated as indicative of "definite" deterioration by Wechsler.

TABLE 2. *Percentages of focally brain damaged patients not identified at the two cutting scores suggested by Wechsler for the Wechsler DQ*

Sample	*N*	*Per cent loss (cut)*	
		Over 10	*Over 20*
Total sample	55	32.73	65.45
NDH subgroup	30	40.00	73.33
DH subgroup	25	24.00	56.00

The mean of the quotients is equal to the quotient of the means made in the derivation of the DQ[6, p. 65].

The DQ also shows a differential effectiveness in the NDH and DH subgroups with the use of these cutting scores, being more effective in the DH subgroup. This difference in effectiveness is statistically significant ($P < .025$) when tested by the chi-square test of frequency discordance. Tables 3 and 4 show the statistics for the Reynell Index. The distributions show even more of a tendency to negative skewness than was true of the DQ distributions; once more the skewness is stronger in the DH subgroup. In contrast to the DQ statistics, it is the DH subgroup which shows the higher mean and standard deviation and the least loss of cases at the ten-point cutting score suggested by Reynell. The laterality subgroup differences are not statistically significant, however. This may be due to the fact that the RI was developed for use with cases of focal injury, whereas the DQ is based on the assumption that the intellectual changes accompanying normal senescent decline are similar to

TABLE 3. *Results of the application of the Reynell index to the total sample of focally brain damaged*

Sample	*N*	*Median*	*Range*	*Mean*	*S. D.*
Total sample	55	17.17	—10-44	14.56	12.16
NDH subgroup	30	14.00	—12-38	13.40	11.48
DH subgroup	25	17.75	—10-44	15.96	12.58

Wechsler does not indicate what proportion of normals may be expected to be misclassified as deteriorated by the DQ, nor has he indicated the means by which he derived the cutting scores he suggests. Since the DQ was derived on the basis of the means of the subtests, it is possible that the actual distribution of the quotient was not prepared. In any case, an estimate of the percentage of false positives to be expected, based on the statistics provided, would necessitate the assumption of normality in the quotient distribution and of subtest equivalence between Forms I and II, as well as the use of the mathematically invalid assumption that

TABLE 4. *Percentages of focally brain damaged patients not identified at the cutting score suggested by Reynell for the Reynell index*

Sample	*N*	*Cutting score*
		10 points
Total sample	55	34.55
NDH subgroup	30	36.67
DH subgroup	25	32.00

those secondary to trauma[6, p. 54]. This may be true, but the adequate psychometric measurement of focal traumatic damage by the DQ may require that the changes be identical, or nearly so, rather than merely similar.

The Pearson correlation of RI with age is not significant, being —.11 in the total sample, —.07 in the NDH subgroup, and —.08 in the DH subgroup. The RI shows moderate to high correlation with the Wechsler DQ. Within this sample of focally brain damaged patients, the correlation between the two indices is stronger in the DH subgroup ($r = +.83$) than in the NDH subgroup ($r = +.65$). The correlation in the total sample is $r = +.74$.

There seems to be sufficient evidence to indicate that both the Wechsler DQ and the Reynell Index leave much to be desired as measures or indicators of focal brain damage. While the RI is less sensitive to laterality localization effects, it appears to have about the same overall effectiveness as DQ which shows far more differential effectiveness in identifying DH and NDH brain damage.

Judging from the present findings, the "twenty-per cent loss" cutting score for the DQ is unquestionably too high, since approximately two-thirds of authenticated cases of focal brain damage fall below this score in the sample studied. The results reported here are consistent with those reported by Allen[1, 2] who has also found the DQ unsatisfactory for use with brain injured patients. Blake and McCarty[3] recently compared a group of normals with Allen's brain injured patients. Though they were able to demonstrate a highly significant statistical differentiation, the separation appeared to be of questionable clinical value.

Fairly consistent evidence appears in the present study to indicate that one factor contributing to the poor clinical performance of present measures of deterioration is that localization effects may interfere with the measurements. This is true because present indices are based on the assumption that brain damage is a single unidimensional variable. It seems possible that a different approach to the problem of deterioration measurement would be profitable. If brain damage varies qualitatively according to the localization of cerebral lesions, several area-specific indices may have to replace single overall deterioration measures.

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THE APPLICATION OF THE WECHSLER-BELLEVUE SCALE IN THE DIAGNOSIS OF ORGANIC BRAIN DISORDERS

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Introduction

This study is an evaluation of various ways of patterning Wechsler-Bellevue subtest scores in an attempt to find the method which most closely agrees with clinical findings and to see whether that method is sufficiently valid to warrant its use in a hospital situation. It is an attempt to evaluate the reliability of the evidence picked up by the use of an instrument not specifically designed for the study of brain damage. Wechsler's patterns[5] are familiar to all clinicians.

Reynell[4] in his formula for detecting organic brain damage uses only the verbal section of the Wechsler-Bellevue scale. He takes the weighted scores for the Information, Comprehension, and Vocabulary tests and gets an equivalent verbal score by multiplying the sum by 5/3 and finding in Wechsler's tables the patient's verbal Intelligence Quotient. A similar procedure is applied to the sum of the Similarities, Arithmetic, and Digit Span tests. If the quotient of the latter, the "Don't hold" tests, is ten points or more below that obtained on the "Hold" tests, a diagnosis of Organic Brain Damage is made.

Hewson[3] worked out a *pattern of deviation ratios* for diagnosing organic brain damage. In this procedure the weighted scores of the Wechsler-Bellevue scale are used in different combinations to make up eight ratios for each set of scores. If a ratio is above a certain point specified by the author, that particular item is termed "organic"; if it is below that point the classification is "normal." (Hewson also has a "neurotic" category which was not included in this study. According to her ratios neurotics score low where organics score high and vice versa, so that the omission of this category in no way influences the results obtained here.)

Table 1 gives the eight ratios and the point above (or below) which each ratio is termed organic.

Certain configurations of *organic* and *normal* individual scores give a diagnosis of "organic" as described by the author. These configurations were worked out by her from known diagnoses on a group of patients. According to her ratios, when

Table 1. *Hewson's deviation ratios*

	Sign	*Ratio*
I. $\frac{\text{Picture Arrangement + Picture Completion}}{\text{Arithmetic + Digit Symbol}}$	Above	1.3
II. $\frac{\text{Information + Comprehension}}{\text{Arithmetic}}$	Above	3.0
III. $\frac{\text{Information + Comprehension}}{\text{Digit + Digit Symbol}}$	Above	1.7
IV. $\frac{\text{Information + Comprehension}}{\text{Picture Arrangement + Digit Symbol}}$	Above	1.6
V. $\frac{\text{Information + Comprehension}}{\text{Digit Symbol}}$	Above	1.1
VI. $\frac{\text{Information + Comprehension}}{\text{Digit Repet. + Picture Arrange. — Digit Symbol}}$	Above	1.1
VII. $\frac{\text{Digit Repetition + Digit Symbol}}{\text{Similarities + Block Designs}}$	Below	.6
VIII. $\frac{\text{Comprehension + Picture Arrangement}}{\text{Digit Symbol}}$	Above	3.0

Ratio I is *organic* and the Digit Symbol score is below the subject's own average weighted score, the record is an organic one; the record is also termed *organic* when any other ratio from I through VIII is organic, unless the only organic one is on Ratio II.

Procedure

In the present study the Wechsler-Bellevue test results of thirty patients diagnosed clinically as *organic brain damage* were analyzed according to the methods given by Wechsler, Reynell and Hewson and which were described in the foregoing. In this way an evaluation of these diagnostic patterns was to be obtained. The subjects were selected at random from the *organic* file of the Neurological Institute of Presbyterian Hospital in New York City. The first thirty cases who had been seen by the Psychology Department were used; seventeen of these cases were tested by the author. Only cases with known organic damage were selected. Patients with a history of concussion were not included in the group.

All subjects had been patients of the Neurological Institute of New York and had had all possible clinical tests including electroencephalogram and pneumoencephalogram to ascertain the diagnosis. Although the possibility of a wrong diagnosis has not been eliminated, it has been reduced to a minimum by various clinical procedures. Five of the patients were diagnosed clinically as convulsive disorder, grand mal type, with some associated deterioration of brain tissue; four were C. N. S. Luetics; thirteen had brain tumors (cerebral) which were subsequently operated; three had an encephalopathy due to poisoning; and five had a congenital brain anomaly. Eighteen of the patients were males, twelve females. The mean age of the group was 29 years, 10 months at the time of testing, and the range from 16 to 54 years. The average Intelligence Quotient for the group was 93 with the range from 62 to 122.

Results

Wechsler does not state how many of the indicators of each patient have to agree with those given in *Measurement of Adult Intelligence* in order to establish brain damage. Consequently, this had to be done by the examiner; in the present study a record was termed *organic* if six or more of the twelve signs agreed with those given by Wechsler. Half a count was given to those items where Wechsler indicated "−−" and the score obtained was "−" or vice versa, with the same procedure applied to the + items. For items which were marked "−− to 0" by Wechsler, any of the three signs, −−, −, or 0 were accepted. Subsequent analysis of data confirmed the assumption that this was a good point of division. As the margin between the percentage of "organics" in the two groups, the one of known brain damage cases and the control group of persons with no known damage, was found to be so small when the method given by Wechsler was applied (as will be seen), adjustment of the number of signs which had to agree could not make a difference large enough to make the result significant.

According to this way of tabulating results, thirteen of the thirty patients in this group were shown to have an organic pattern and it can be said that in thirteen, or forty-three per cent of the thirty cases, correct diagnosis would have been made by the Wechsler method of patterning. Thus we see that the signs are of little significance from a clinical point of view; it would become apparent that they cannot be used for differential diagnosis.

Using Reynell's method fifteen of the thirty patients are classified in the *organic* group making a correct diagnosis in fifty per cent of the cases which, though higher than that obtained by the Wechsler method, is still not valuable in actual diagnosis. According to Hewson's ratios eighteen patients, or sixty per cent of the cases, fell in the organic group.

Table 2 shows the distribution of organic diagnosis of the different patterns on the basis of the patient's clinical diagnosis.

In examining this chart, no one method stands out markedly in being better or decidedly poorer than the other two methods for diagnosing a particular classification; that is, whereas one method,

TABLE 2. *Distribution of "organic" classification on the basis of the various patterns*

Classification	*N*	*Organic*			*Not organic*		
		W	*R*	*H*	*W*	*R*	*H*
Grand Mal	5	1	2	3	4	3	2
C. N. S. Lues	4	3	2	3	1	2	1
Tumor	13	5	6	7	8	7	6
Congenital and Birth Injury	5	2	4	4	3	1	1
Toxic Encephalopathy	3	2	1	1	1	2	2
	30	13	15	18	17	15	12

W = Wechsler. R = Reynell. H = Hewson.

that given by Hewson, gives a higher percentage of accurate diagnoses, it cannot be said that it is better in any one particular category, such as picking out brain tumor cases, but rather that the general picture seems to be somewhat better than it is on the other two methods.

CONTROL GROUP

A control group of thirty "normal" cases was patterned next according to the procedures described. This group was selected at random from the files of the New York University Testing and Advisement Center; all cases were those of clients who had come to the Center for vocational guidance. They were used in an attempt to see the incidence of "organic" diagnosis in a group of people who have shown no organic signs whatever, and in whom we have no reason to suspect organic brain damage.

In this group there were twenty-two males and eight females; their ages ranged from 17 years, 8 months to 52 years, 2 months, the median age being 25 years, 4 months. The range of Wechsler-Bellevue quotients was from 101 to 133 with the median 117.

Relationship between Intelligence Quotient and agreement of findings with Clinical Diagnosis could not be established except for the fact confirmed in the literature by Allen[1] that patients with brain damage tend to score consistently lower than those without such damage. It was not possible to find a relationship between age and correctness of diagnosis; thus it is thought that the differences of age and Intelligence Quotients of the two groups used do not interfere with the direct comparison of results.

According to Wechsler's method of patterning, ten, or thirty-three per cent of the thirty "normal" cases, fell into the organic group. Applying Reynell's method, nine cases, or thirty per cent, are termed organic; Hewson's deviation ratios show five, or seventeen per cent such records.

DISCUSSION

The methods of diagnosing cerebral pathology devised by Wechsler, Reynell and Hewson showed the presence of such involvement in 42, 50 and 60 per cent respectively of a group of patients who were shown clinically to have an organic involvement of the brain. Since it is probable that cases with a doubtful clinical diagnosis who do have a cerebral pathology would not give a pattern as characteristic of this classification as do those whose diagnosis was clinically evident, we may expect the percentage of valid diagnoses to be even lower in clinically doubtful cases.

All three of the methods agree in their conclusions regarding the statement of whether or not the patient's intellect has been affected by an organic disease of the brain in only ten cases (thirty-three per cent) of the total number of cases analyzed. When we consider that of these nine cases, five (17 per cent) of the total number, were considered "not organic" by all three methods, the percentage of correct diagnosis is even lower than would seem at first. It is felt that the possibility of a wrong clinical diagnosis on any of these five cases may be eliminated as definite evidences of pathological lesions were found.

Tabulating the results of the control group with those obtained on the group

of patients known to have an organic pathology (Table 3) makes possible a comparison and estimation of the actual margin of validity of the three procedures.

TABLE 3. *Comparison by percentages of organic classification*

Method	*30 cases known to have a cerebral pathology*	*30 cases with no evidence of such damage*
Wechsler ...	43%	33%
Reynell	50%	30%
Hewson	60%	17%

We thus find that the chances of making a correct diagnosis on the basis of Wechsler's patterns are .55, that is, that we will be correct 55 times in a hundred when applying this method of diagnosing organic brain disease. Chi square is .63, showing that these results might have been obtained by chance 5 times in ten, thus making no gain over pure guesswork.

With Reynell's method the chances of making a correct diagnosis are .60; Chi square is 2.5 with one chance in ten of obtaining for results by chance. The chances of making a correct diagnosis by the Hewson method are. .717; in seven out of ten cases who are "organic" according to Hewson's method, clinical neurological examination confirmed this conclusion. Chi square for this method is 5.9, showing that there is less than two chances in a hundred of getting the results by chance. From the foregoing it appears that the methods given by Wechsler and Reynell are ineffectual in differential diagnosis. Hewson's deviation ratios, although they should not be taken by themselves as a means of differential diagnosis, do increase the chance correct diagnosis considerably.

SUMMARY

This study is an attempt to evaluate three methods of using Wechsler-Bellevue scores for differential diagnosis in cases of organic brain damage. The Wechsler-Bellevue scores of the experimental and clinical groups were analyzed according to methods given in the literature by Wechsler, Reynell and Hewson in an attempt to find which of the methods, if any, were feasible for use in differentiating between patients who have cerebral brain damage from those who do not.

It was found that the only method feasible for actual clinical use is that developed by Hewson which agreed with clinical diagnosis in 71 per cent of the cases used. From the results obtained in this study it would seem that the Hewson Deviation Ratios are sufficiently helpful in the diagnosis of organic brain damage to warrant their use together with other psychological tests and clinical procedures. It should be pointed out, however, that we cannot expect the method to be as helpful in clinically doubtful cases; a disease of the brain must bring about a sufficiently great change in personality before it can be detected by psychological tests of intelligence or personality.

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A COMPARATIVE EVALUATION OF THE WECHSLER-BELLEVUE MENTAL DETERIORATION INDEX FOR VARIOUS ADULT GROUPS*†

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In recent articles the value of the Wechsler Mental Deterioration Index (MDI)‡ in screening patients with brain damage from normal patients has been considered. Allen[2] has found that by using a loss of greater than 20%, he could definitely screen out only 54% of his study group of 50 patients. Blake and McCarthy[3] raised the question as to how many normal subjects would be misclassified as showing impairment by the use of such an index. In their paper they were concerned solely "with the problem of distinguishing qualitatively different preselected groups at the quantitative level by using this index, and not with its value as a technique for differential diagnoses." However, they note that Allen implied the use of this index for differential diagnoses, as did Levi, et al.[6]

The purpose of this paper is to investigate the MDI further as a tool for differential diagnosis; that is, to determine whether a significant loss in this index is related solely to deterioration such as is found in subjects who have had organic brain injury or damage. Levi, et al.[6] pointed out that "the index of deterioration is useful not only in confirming, but also in discovering organic conditions." However, Wechsler[11, p. 67] points out that this index may also be applied in cases of schizophrenia, although he goes on to say that schizophrenics would tend to have lower scores on some of the "Hold" tests than on some of the "Don't hold" tests. If this index can be considered as a tool for distinguishing subjects with organic brain injury from all other categories of subjects, then in the other categories the MDI should be insignificant. The continued use of the MDI in its present form gives pertinence to a study such as this.

Subjects

This report is based on a total of 349 subjects. They were all given the Wechsler-Bellevue Form I examination, and fall into seven distinct groups, four of which were previously reported in the literature. Mental deterioration indices have been reported on for two groups, and were computed by the author for the rest. Allen[2] reported on 50 brain injured patients, that is, patients who had suffered actual penetration into the encephalon. Blake and McCarthy[3] compared normal subjects, a group of 50 college students, to Allen's group. In this paper we have also used the normal control group as reported by Rapaport[9]; we used only the 49 men who fell into the well-adjusted group and border-line adjusted, and eliminated the five maladjusted individuals. Kogan[5] reported test results of 50 patients who were clear-cut cases of schizophrenia at the time of test administration, and were in treatment at a mental hygiene clinic or in a neuropsychiatric hospital, and who also had a previous record of a diagnosis of schizophrenia. In addition to these 199 subjects, three other groups of 50 each were added. Two groups were obtained from the files of a vocational counseling service which had a contract with the Veterans Administration. One of these, VCS non-neurotic, consisted of 50 men who were receiving compensation for some non-neuropsychiatric injury incurred in or aggravated by military service. These disabilities ranged from malaria to TB to gunshot wounds. The VCS neurotic group consisted of 50 men who were receiving compensation for neuropsychiatric conditions. Both of the VCS groups were making application for voca-

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‡ All MDI's are corrected for age.

TABLE 1. *Differences in MDI's for the groups*

Subjects	Mean Age	Mean IQ	No loss N	No loss %	Less than 10% N	Less than 10% %	10-19% N	10-19% %	20% or more N	20% or more %
Allen	25.0	94.98	8	16	6	12	9	18	27	54
Blake and McCarthy	28.3	124.80	20	40	18	36	8	16	4	8
Rapaport	35.76	115.70	22	45	14	29	12	24	1	2
VCS Non-Neurotic	27.92	101.11	17	34	9	18	11	22	13	26
VCS Neurotic	28.12	105.12	19	38	10	20	7	14	14	28
MHC Neurotic	28.20	110.01	11	22	13	36	11	22	15	30
Kogan Schizophrenic	25.38	105.00	13	26	14	28	12	24	11	22

tional training under Public Law 16 at the time the tests were administered during the calendar year of 1946. The groups were differentiated only on the basis of Veterans Administration diagnosis, not on the basis of current complaints or symptoms. It was noted at the time of testing that many of the complaints of the "non-neurotic" group seemed to be of psychogenic origin, and that as a group they appeared to be quite maladjusted. For the most part these men were recently discharged from service and were unemployed. The results reported later indicate that as a group they resembled the maladjusted much more than the normal subjects. Both of these groups were selected in order to match Allen's group most closely so far as age and IQ were concerned. The MHC neurotic group consisted of 50 veterans who had made application for and had received treatment in a Veterans Administration mental hygiene clinic, where they had been diagnosed as anxiety tension state. These cases were selected at random. The mean age and mean IQs for all groups will be found in Table 1. For every group except Rapaport's, the N equals 50, and in Rapaport's, the N equals 49.

RESULTS

The basic data are presented in Table 1. The first row is reproduced from Allen[2], and the second row from Blake and McCarthy[3]. These data are in line with other studies on the MDI; Garfield and Fay[4] and Magaret and Simpson[8] who studied the relationship between the Wechsler-Bellevue MDI and the Shipley-Hartford CQ for patients with various diagnoses report 21% and 14%, respectively, of their patients had MDI's of over 20%.

Table 1 shows consistent differences on the one hand between Allen's group of the brain injured, and the two normal

TABLE 2. *Comparison of the significance of the difference between the groups*

	Allen	Blake & McCarthy	Rapaport	VCS Non-Neurotic	VCS Neurotic	MHC Neurotic	Kogan Schizophrenic	Normal Curve
Allen	..	.01	.01	.05	.02	.10	.05	.01
Blake & McCarthy	..	..	.50	.05	.05	.02	.10	.70
Rapaport	..	..	..	.01	.01	.01	.02	.50
VCS Non-Neurotic	..	..	..	..	.80	.70	.50	.10
VCS Neurotic	..	..	..	..	..	.20	.20	.10
MHC Neurotic	..	..	..	..	..	..	.80	.01
Kogan Schizophrenic	..	..	..	..	..	..	..	.10
Normal Curve	..	..	..	..	..	..	..	..

TABLE 3. *Use of a critical score of 10 to differentiate groups*

Subjects	Less than 10		10 or more		Approx. ratio
	N	%	N	%	
Allen	14	28	36	72	1:3
Blake & McCarthy	38	76	12	24	3:1
Rapaport	36	73	13	27	3:1
VCS Non-Neurotic	26	52	24	48	1:1
VCS Neurotic	29	58	21	42	1:1
MHC Neurotic	24	48	26	52	1:1
Kogan Schizophrenic	27	54	23	46	1:1

groups (Blake and McCarthy, Rapaport). On the other hand, the brain injured group also shows consistent differences from the other four categories. In short, the brain injuries seem to fall in a special category. This is illustrated by Table 2, showing the P values for the χ^2 scores. So far as Allen's group is concerned, it differs at the 1% level of confidence from the normal groups, and at levels of confidence from 2% to 10% from the other groups.

It is interesting that the normal groups do not differ at all from each other, but that both of them differ markedly from all the other groups, ranging from the 2% to 10% level of confidence. The four "maladjusted" groups when compared with each other by the χ^2 method, indicate that the hypothesis that the differences between them are due to chance must be retained. Apparently, then, the specific type of diagnosis does not matter; with maladjustment the MDI goes up.

Blake and McCarthy point out that the practical efficiency of the MDI may be considered, using a score of 10 as a critical score rather than the 20% criterion usually described. Using this score (see Table 3) they find the ratio of correct classification to error is approximately 3 to 1 for both the brain injured and the normal subjects. Thus, they find that only 26 of the 100 subjects would be incorrectly classified, while 74 would be appropriately designated. For our additional normal group (Rapaport) this ratio holds true; only 27% would be incorrectly classified. However, when we apply this technique to those groups with functional disorders, we find that it is no better than chance and that for each of the four groups, approximately half of the patients would be incorrectly classified, so that while this index has some value in differentiating the brain injured from the normals, it has no value in differentiating the brain injured from subjects with functional disorders.

Wechsler[11] has indicated that 10% to 20% losses are roughly equivalent to 1 and 2 PE from the mean, but he fails to offer any evidence in support of this. If this is so, then we can expect our normal groups to fall roughly in the same pattern. The χ^2 (see Table 2) was computed comparing each of the groups with the assumed normal distribution of MDI's. These results indicate that the probabilities of the differences being due to chance were .70 and .50 for the normal groups; .10 for the two VCS groups and the schizophrenic group, and at the 1% level of confidence for the MHC neurotic and the brain injured. It would appear then that the MDI's for the normal subjects do not differ from the standardization group, that the chances are 9 out of 10 that the difference found for the VCS groups and the schizophrenics are not due to chance, and the differences found for the neurotics and the brain injured are significantly different from chance.

SUMMARY

1. The MDI's for the following seven groups of subjects were compared: brain injured, applicants for vocational counseling with psychoneurotic diagnoses, and with other than psychoneurotic diagnoses, mental hygiene clinic patients diagnosed as anxiety tension state, mental hygiene and hospital patients diagnosed as schizophrenic, and two groups of normals.

2. This report agrees with a previous study in that the brain injured subjects

differ as a group from the normal. The brain injured group also differs from all the other groups except the anxiety tension state groups to a significant degree. In general, the two well adjusted groups differ from all the other groups significantly (except in one instance) but are themselves similar. While the four maladjusted groups differ from both the brain injured, and the well adjusted, they show a high degree of similarity to each other.

3. When a critical score of 10 for the MDI is used, this criterion will correctly classify approximately 75% of the subjects, when only the brain injured and the normal groups are considered. However, for the other four groups, the results are no better than chance; approximately 50% of each group would be incorrectly classified.

4. It was found that the MDI's for the normal subjects did not differ to a degree greater than that expected by chance from a normal distribution of MDI's. This supports Wechsler's contention that MDI's are distributed along a normal curve with 10% and 20% at 1 PE and 2 PE from the mean, respectively.

5. Magaret[7] said at the conclusion of one of her studies of the Wechsler-Bellevue, "The only conclusion warranted is that certain tests of the scale are sensitive to whatever changes in the organism which occur similarly in schizophrenics, general paretics, and old age." To which we might add the additional categories of the brain injured and the neurotic and the maladjusted individuals, in so far as the change is measured by the MDI.

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A NOTE ON ALLEN'S INDEX OF DETERIORATION*

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Allen[1] recently suggested a new index to replace the Mental Deterioration Index (MDI)† of the Wechsler-Bellevue. He suggests computing the difference between the sum of the Information and Comprehension weighted scores and the Digit Span and Digit Symbol weighted scores. A difference of five or more points he considers as being suggestive of brain injury. He did not compute an individual index for each of his own patients, but derived it from his table of mean subtest scores for brain-injured patients.

Blake and McCarthy[2] using Allen's proposed index with normal subjects found it would not be more desirable than the MDI since "it would result in an increment in the amount of classification error with normal subjects." The present author[3] recently reported a study on the MDI with normal subjects, applicants for Vocational Rehabilitation both with the diagnosis of neurosis or with diagnoses other than neurosis, with schizophrenics, and with anxiety tension state patients. A total of 349 subjects were used in this study. In this connection Allen's index was also computed and the results are shown in Table 1. It was found that the approximate ratio of correct to incorrect classification for normal subjects was 3:1 using as a critical score an MDI of 10[3]. This ratio fell to 2:1 using Allen's index. For the various types of maladjusted subjects neither index could produce more than a 1:1 ratio.

TABLE 1. *Classification of subjects using Allen's Index*

	Less than 5		*5 or more*	
	N	%	*N*	%
Blake & McCarthy (Normal)	33	66	17	34
Rapaport (Normal)	32	65	17	35
VCS (Non-Neurotic)	29	58	21	42
VCS (Neurotic)	28	56	22	44
MHC (Neurotic)	21	42	29	58
Kogan (Schizophrenic)	29	58	21	42

In summary our results confirm those of Blake and McCarthy. Allen's index would be somewhat less discriminative than the MDI so far as normal subjects are concerned. Neither index produces significant results with maladjusted groups.

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† All MDI'S are corrected for age.

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EDITORIAL OPINION

This *Journal* is gratified to have the opportunity to publish the complete Symposium on Psychology in the Field of Community Services presented in September 1949 at the Denver meeting of the *American Psychological Association.* The fact that such a symposium was scheduled gives indication of the increasing awareness of the potential contributions of scientific psychology to community planning and service. In a world unsettled by the Four Horsemen of the Apocalypse, and threatened by the still more dreadful spectre of atomic warfare, our best hope for preventative measures seems to come from the promise of the psychological and social sciences which have temporarily been eclipsed by the spectacular accomplishments of the physical sciences. One of the ideas suggested by all the papers of this symposium is that psychology as science will have failed to live up to its potential contribution until it begins to make important practical contributions to the group dynamics of everyday living. Important though science may be for its own sake, will it not be ultimately sterile if it fails to save the world from the mad whirl of aggression and destruction which we are now witnessing? Probably the greatest need of civilization is for the rapid and effective dissemination of psychological principles in the conduct of its affairs, but how is psychological science to accomplish this end?

The current impotence of scientific or applied psychology may be illustrated by examples all the way from family life to world politics. Although the current war of nerves being conducted between the East and the West undoubtedly involves effective psychological principles, it is to be regretted that the negative emotions of fear and hate appear to be the basic weapons in a cat-eat-dog, eye-for-an-eye conflict. To be certain the Western powers visualize themselves as being more Christian and enlightened, but there is no certainty that the concepts of pacifism and forgiveness will be effective in preventing another catastrophe. One of the most profitable research investigations which could be conceivably made might be the study of how psychological scientists could gain a hearing for themselves, i.e., how to intervene potently.

One of the first problems encountered by the psychologist entering the field of community service either on local or international levels is the fact that *others are already in administrative control of the situation.* Most community affairs are currently being run by people who by virtue of experience and power feel that they know best how to do things. Indeed, the psychologist is a belated comer to the field who will probably find himself equally ill at ease whether he attempts to help in the affairs of a village or on the international stage. Perkins is correct when he stresses the fact that the psychological scientist should enter the scene as a "listener and learner." He will soon learn that his psychological learning has relatively little to do with the problem of how to raise wages, improve working conditions, and provide community resources for dealing with mental hygiene problems. In fact, many of the laymen with whom he will have to work will turn out to be extremely shrewd citizens who are masters of the practical psychology of the local situation. The psychologist can expect that his training and academic prestige will cut little ice unless he can overcome the natural suspicion of the layman for "long-hairs" and actually make himself felt in terms of effective performance. The first step in accomplishing this is simply to be around long enough for people to become accustomed to the presence of a psychologist in their midst. During this initial getting-acquainted process the psychologist should tread softly since there will be many insecure people who will feel threatened by their belief that he can read their minds, and otherwise recognize them as mentally unfit or disordered. Above all should the psychologist not be led into trying to "over-sell" himself because of his own feelings of insecurity.

One of the first reactions commonly ex-

perienced by the apprentice psychological or social worker is a feeling of disgust or revulsion on discovering just how bad some conditions are. Whenever a new welfare service enters a community, there are immediately referred to it a collection of community "sores" and human derelicts. Typical problems include the alcoholic pauper with 18 children who only stops at home long enough to beget another, the mentally defective family which has been the town's chief welfare burden for the last 75 years, the urban or rural slums from which comes most of the delinquency, and the incestuous parent or sex criminal who has been sent away several times only to be pardoned so as to repeat his offenses. The beginner's first reaction is that such conditions are intolerable and something must be done right away. Having learned all the answers in school, he will usually come up with an expensive plan for which there are no resources in the community to execute. Being young, he may raise such a public furore that some community agency is stimulated to make abortive efforts to resolve the situation but these are usually destined to failure because the community as a whole has neither the insight nor the resources to carry them out. Too often, this dramatic publicity only succeeds in making the citizenry feel uneasy at having its dirty linen washed in public with the result that the clamor soon dies down and the welfare enthusiast finds that he has lost his usefulness by accomplishing no more in the end than many other zealots before him.

Zander makes a most important point in this symposium when he calls for a coordinated research program to tackle the problem of how best to organize mental hygiene preventative services. Such a program must function at all levels from the selection of students in universities, to the planning of training programs, and to the practical details of how to put an effective program across in the community. *A most important advance would be made if every psychologist made himself a committee of one to actively participate in the affairs of his community and to make whatever contribution he could.* Perhaps we psychologists do not know the answers at the present time, but at least no other group in the community has comparable academic and research training in problems of interpersonal relations. Such a program would not only make a contribution to community services, but it would also improve the psychologist's own practical abilities and give him some first hand experience of what the problems actually are.

F.C.T.

The status of research evaluations of the results of counseling and psychotherapy would be materially advanced if adequate attention was given in research planning to problems of diagnosis. It is obvious that no rational evaluation of therapeutic results can be achieved in the absence of knowledge concerning the nature of the pathological process which it is intended to modify. Without a valid diagnostic formulation it is even impossible to state that any discernible pathological state actually exists. Similarly, without diagnostic information concerning the type of case material dealt with, it is impossible to reproduce the conditions of the experiment elsewhere or even to analyze the results. All sorts of therapeutic claims may be made if the nature of the case material is unknown. In other words, failure to make adequate diagnostic studies may completely invalidate research results because of failure to identify and control the variables involved.

Medical history yields pertinent analogies on the difficulties of assaying therapeutic claims and also indicates possible avenues of solution. A good example of how medical research has handled the comparative study of different methods of therapy is the problem of whether X-ray or surgery is the method of choice in treating certain types of cancer. For years controversies raged in support of either method with entirely confusing and inconclusive results. The inconsistency of results was caused by the failure of research planning to conduct sufficiently detailed diagnostic studies to control the sampling of cases adequately. Failure to differentiate between various degrees of malignancy of the tumors resulted in the attempt to relate incomparable groups, e.g., one study reported results with cases of low malignancy while another research studied cases

of high malignancy with entirely different results.

In order to make an exact differentiation of the case material under study, it is necessary not only to differentiate the nature of the pathological process but also its malignancy. Assuming that current diagnostic categories are reasonably satisfactory in differentiating the various clinical syndromes, attention may be given to quantification of the degree of malignancy. It would seem desirable to use the following criteria for estimating malignancy:

1. *Pathogenicity* of the etiologic factors.
2. *Organicity.* In general, organic disorders are more malignant than functional. It is of course important to differentiate between the malignancy of different organic disorders.
3. *Chronicity.* In general, the prognosis becomes worse with increasing chronicity.
4. *Direction of Change.* Increasing severity of incapacitation is an undesirable sign.
5. *Client Resources.* The greater the personality resources, the better the prognosis.
6. *Client Insight.* Lack of client insight is a poor prognostic sign.

For a valid research plan, it would appear necessary to make refined enough diagnostic studies in order to differentiate the nature and malignancy of the case material studied.

This issue contains a solicited evaluation by Albert Ellis of the nondirective research project "A Coordinated Research in Psychotherapy" appearing in the June 1949 issue of the *Journal of Consulting Psychology.* In our opinion, the viewpoint of the nondirective group that adequate diagnostic studies are unnecessary for effective therapeutic results has largely vitiated the validity of most of the research thus far published in support of the nondirective method. In the absence of careful diagnostic studies of the case materials, or at least descriptions of the presenting complaints with psychological appraisals of the personalities involved, we are unable to tell enough about the nature of the case material to draw any conclusions. Unless the fact of disorder and its malignancy is established, it is only safe to conclude that these cases either had no significant personality problems or else that they were of very superficial nature which almost any method might cure. It will not be possible to evaluate the results from any method of counseling and psychotherapy until valid research planning is achieved.

F.C.T.

The June 1949 issue of the *American Psychologist* contains a report by Keller and Schoenfeld about a new psychology curriculum at Columbia College. Because of the prestige of the Columbia department and the influence which it has traditionally carried in American psychology, it appears necessary to evaluate the new curriculum with particular reference to its possible effect upon the healthy development of clinical psychology. In order to understand the historical antecedents of this new approach, it may be remembered that for many years the Columbia department has been a stronghold of Behaviorism and "objective" psychology in the sense that its orientation was preoccupied with "pure" science and experimentalism. Since only the simpler mental processes were amenable to brass-instrument experiments, there has been a tendency at Columbia to avoid the study of the organism at the highest levels of function, i.e., the whole individual meeting its complex environment. Clinical psychology has never gained a respectable foothold either in Columbia College or the graduate department but has been delegated to Teachers College. After World War I there were several flickerings of interest in clinical psychology in individual staff members but they were never allowed to develop their plans, hence the Columbia department has never developed clinics and other facilities for securing case materials for research and teaching purposes. The result has been that generations of Columbia students have obtained the highest graduate degrees without ever having come face-to-face with a single human case for the purpose of making an intensive personality study. Columbia graduates are unquestionably well trained in experimentalism and research technique but their training which was largely concerned with basic process and "rat psy-

chology" had large blind spots relating to problems of total personality. In justifying the narrowness of this outlook, the Columbia staff has argued that the student who was well trained in basic research was automatically well equipped to enter any other field. The validity of this opinion has for years been questioned by a number of graduates who have been professionally handicapped by lack of specialized training particularly in the field of "the whole individual in action." Personally, we believe that it is unwise on both philosophical and scientific grounds for any university department to subscribe to any "systematic" viewpoint whether it be Behaviorism or psychoanalysis.

In our opinion, any broad educational program in psychology must represent a compromise between many objectives, not the least of which is to give the beginning student enough of the applied psychology of everyday living so that he is able to understand the potential contribution of psychology to civilized living and to solve his own personal problems. It must be clearly recognized that very few college students will ever become research scientists nor is it desirable to give them intensive training in basic science at the expense of applied psychology. Even more on graduate levels of instruction is it unwise to largely ignore such important areas as that of clinical psychology. Very few students can predict the direction of their later interests. Nor can a department limit itself to the production of "scientists." We believe that the current fashion of distinguishing between "pure" and "applied" scientists is spurious and artifactual. The psychologist of the future must be well-rounded. In order to accomplish this, training programs must be genuinely broad, covering both basic and applied science. We question very seriously whether a curriculum oriented at the level of the rat can qualify as being genuinely broad. We can conceive that some students passing through the type of curriculum outlined by Keller and Schoenfeld might come out with a broad viewpoint but if such occurred it would be in spite of the type of training offered. The brilliant student will always explore human knowledge for himself in spite of the "systematic" viewpoint of his teachers. To us, such a program represents a regression rather than an advance. F.C.T.

BOOK REVIEWS

SYNGG, DONALD and COMBS, ARTHUR W. *Individual behavior*. New York: Harper, 1949, pp. 386. $3.50.

This is a timely, well-written, and interesting book to which no brief review can possibly do justice. Its main strengths and weaknesses, therefore, will be summarily outlined. First, as to strengths:

1. It presents a consistent, if at times oversimplified and almost glib, picture of the human organism psychologically reacting with its environment; and it does so without the obnoxious folklore, dogmatism, and mystical attitudes of many orthodox psychodynamic writers.
2. It re-emphasizes, with logical persuasiveness, the vital importance of studying human behavior and its disorders from the frame of reference of the patient rather than from arbitrary external frames of reference.
3. It ably shows how individual frames of reference may be valuably applied to problems of conditioning, school learning, perception, and other fields of psychological study which have up to now been too externalized in their approach.
4. Clinically, it places a justifiable emphaiss on the patient's *attitudes* towards his environment, his problems, and his general behavior rather than only on his behavior itself.
5. Its chapters on education and teaching are particularly good in that they ask the pertinent question: *Education*

for what? and unequivocally answer: For the development of each child's "maximum potentialities as an individual and a citizen" (p. 224).

6. Unlike most contemporary non-directively oriented writing, it is sufficiently broadminded to leave a place, albeit an underemphasized one, for diagnosis, case history taking, and even direct manipulation of the patient's environment.

Some of the main weaknesses of *Individual behavior* are these:

1. It gives an impression of more originality than it actually posessses. Although its *expression* of psychodynamic theory is novel, it adds little new knowledge of human behavior, and few very deep or penetrating formulations are presented. The phenomenological approach has apparently failed to reach, as yet, the depth analysis of the psychoanalytic pioneers on whose discoveries Snygg and Combs so fundamentally rely.
2. The constructs employed by the authors, while simpler, probably more scientific, and certainly more in line with conventional psychological thinking and terminology than many of the constructs and myths of orthodox analytic theory, are still hypotheses which seem to be at least one step removed from clinical observations. *Phenomenal field* and *phenomenological self* are more operationally definable and easier to work with than *ego, id,* and *superego;* but they still seem to be redundant, highfaluting approximations for the *human organism* and the *external world.* Thus, Snygg and Combs state that "All behavior, without exception, is completely determined by and pertinent to the phenomenal field of the behaving organism. By phenomenal field we mean the entire universe, including himself, as it is experienced by the individual at the instant of action" (p. 15). What this seems to mean, more simply stated, is that all human behavior is determined by (a) the human individual interacting with (b) the outer world. The ten-dollar terms, *phenomenological self* and *phenomenal field,* so reiteratively employed by the authors, seem to add nothing but some degree of sematic difficulty and obfuscation.
3. The weakest section of the volume is that concerned with therapy—which contains nearly all the contemporary biases and limitations of the non-directive viewpoint. While ably pointing out the value of non-directive techniques for *some* disturbed individuals, the authors imply (a) that these techniques are effective with virtually *all* types of patients; (b) that they are *more* effective with all patients than all other forms of therapy; and (c) that there are no patients who do not have powers of self-recovery right at their fingertips if only they are non-directively counseled. Fortunately, the main arguments of the volume do not stand or fall with the authors' non-directive applications. But the danger is that many unsophisticated students of psychology who come to have faith in the authors' main theories may blindly follow their superficial and inadequate treatment of the therapeutic process.
4. The volume frequently sets up strawmen of "externalists," "inductionists," and other psychologists who are accused of completely ignoring, or even sabotaging, the private worlds of normal human beings, and of acting in a thoroughly non-scientific manner in which, in fact, virtually no contemporary psychologists seem to act.
5. *Individual behavior* is clinically weak at several points. Thus, it speaks about phobias as if they were clearly differentiated from neuroses (p. 159); it describes as "manic depression" what seems to be a fairly clear-cut case of reactive depression (p. 168); and it implies that delusions of grandeur are intrinsic only to hebephrenic, and not to paranoid, schizophrenic states (p. 174).

In conclusion, Snygg and Combs' *Individual behavior* consists of interesting and intelligent reformulations of many psychodynamic viewpoints; and while it is hardly a definitive volume on human behavior and therapy, it is well worth the serious, and

it is to be hoped critical, consideration of clinical psychologists. ALBERT ELLIS

SARASON, SEYMOUR B. *Psychological problems in mental deficiency.* New York: Harper, 1949. Pp. 366. $5.00.

Dr. Sarason is Associate Professor of Psychology at Yale University and formerly psychologist at the Southbury (Conn.) Training School for mental defectives. There has long been need for a scientifically oriented textbook in the field of mental deficiency. This book eminently satisfies the need. It is readable, up-to-date and with a judicious selection of research reports. Of particular value are the detailed discussions of the handling of mental defectives.

WALLIN, J. E. WALLACE. *Children with mental and physical handicaps.* New York: Prentice-Hall, 1949. Pp. 549. $5.00.

Dr. Wallin is one of the pioneers in America in the field of special education. His authoritative textbook will probably supplant Tredgold's classic work on mental deficiency.

BRIDGE, EDWARD M. *Epilepsy and convulsive disorders in children.* New York: McGraw-Hill, 1949. Pp. 670. $8.50.

Dr. Bridges is Research Professor of Pediatrics, School of Medicine, University of Buffalo. This is a broad reference textbook covering all aspects of convulsive disorders, Its orientation is thoroughly scientific in all respects.

WALLIN, J. E. WALLACE. *Personality maladjustments and mental hygiene.* New York: McGraw-Hill, 1949. Pp. 581. $5.00.

This is the second edition, completely revised and enlarged, of Dr. Wallin's well-known text. Appropriate for undergraduate and elementary graduate courses in mental hygiene, it is very well documented and easily readable.

LEMKAU, PAUL V. *Mental hygiene in public health.* New York: McGraw-Hill, 1949. Pp. 396. $4.50.

Dr. Lemkau is Associate Professor of Public Health Administration at Johns Hopkins University. He discusses the practical public health problems concerned with establishing mental hygiene programs for all periods of life.

SODEN, WILLIAM H. *Rehabilitation of the handicapped.* New York: Ronald Press, 1949. Pp. 399. $5.00.

Mr. Soden is Executive Assistant, Physical Medical Rehabilitation Service, Veterans Administration Hospital, Northampton, Mass. The book discusses general medical and surgical, neurological, psychiatric, vocational, social, educational and psychological trends in rehabilitation of the handicapped.

WEVER, ERNEST G. *Theory of hearing.* New York: Wiley, 1949. Pp. 484. $6.00.

Dr. Wever is Professor of Psychology at Princeton University and a pioneer researcher in the field of hearing. This is probably the most authoritative reference work in this field available today.

CHAPANIS, A., GARNER, W. R. and MORGAN, C. T. *Applied experimental psychology.* New York: Wiley, 1949. Pp. 434. $4.50.

The authors are all members of the psychology department at Johns Hopkins University. It reviews experimental findings related to the human factors in engineering design.

CAVAN, R. S., BURGESS, E. W., HAVIGHURST, R. J. and GOLDHAMER, H. *Personal adjustment in old age.* Chicago: Science Research Associates, 1949. Pp. 204.

This is the report of a study conducted by a committee on Social Adjustment and Old Age supported by the Social Science Research Council on a group of 2,988 older persons. Methods and results of research approach are discussed.

FREUD, SIGMUND. *An outline of psychoanalysis.* New York: Norton, 1949. Pp. 127. $2.00.

This is the first English translation in book form of Freud's last book written in 1938. It is a brief resume of the principles of psychoanalysis.

ALEXANDER, FRANZ. *Fundamentals of psychoanalysis.* New York: Norton, 1949. Pp. 312. $3.75.

This book represents a revision of portions of *The Medical Value of Psychoanalysis* reviewing the basic concepts of psychoanalysis. It includes a theoretical refinement of the integrative functions of the ego in terms of the stability principle of Freud, the inertia principle and the principle of surplus energy.

VARIOUS EDITORS. *The psychoanalytic study of the child.* Vols. 3-4. New York: International Universities Press. 1949. Pp. 493. $10.00.

This volume includes a selection of original psychoanalytically-oriented studies on the child.

MOLONEY, JAMES CLARK. *The magic cloak.* Wakefield (Mass.): Montrose Press, 1949. Pp. 345. $5.00.

This is a collection of essays by a psychiatrist of wide experience. The subject matter includes anecdotes from his work and personal reflections on psychiatric and psychoanalytic matters.

RAMBERT, MADELEINE L. *Children in conflict.* New York: International Universities Press, 1949. Pp. 214. $3.25.

This is a popular book for parents and teachers summarizing 12 years of psychoanalytic practice.

HILLPERN, E. P., SPAULDING, I. A. and HILLPERN, E. P. *Bristow Rogers: American Negro.* New York: Hermitage Press, 1949. Pp. 184. $3.00.

This is a psychoanalytic case history written in prose style.

FODOR, NANDOR. *The search for the beloved.* New York: Hermitage Press, 1949. Pp. 400. $5.00.

This is an analytically oriented attempt to trace the effects of the birth trauma in man. Some case material is given with author's interpretations.

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SCHEDULE. The Workshops will be confined to morning hours from 8:30 A.M. to 12:30 P.M. from Monday through Saturday. There will be approximately 25 hours of intensive work and discussion in each Workshop. The afternoons will be left free for rest or recreation.

DATES. Final dates will depend upon convenience of registrants. Tentative dates are: Course I, June 19-24, July 17-22 or August 21-26. Course II, June 26-July 1, July 24-29 or August 28-September 2. Please indicate preference when registering.

REGISTRATION. Will be limited to professional psychologists and students beyond the level of the Master's degree. It is expected that each registrant will have had sufficient clinical experience to be able to contribute to the Workshop. Only a limited number will be accepted at each Workshop.

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